THE

Philosophical Review

July 1956

Conditions of Observation and States of Observers *R. Ziedins* 299
A Criticism of Plato's *Cratylus* .... *Richard Robinson* 324
Sense and Reference: A Reply .. *William Marshall* 342
The Concept of Empirical Knowledge *Arthur Smullyan* 362
Discussion
Physics and Structure ..... *Robert Palter* 371
Regular Rules of Induction ..... *Wesley C. Salmon* 385
On Being Ideal ..... *Richard G. Hansen* 389
Concerning the Language of Religion.... *C. J. Ducasse* 401
Reviews 403
Books Received 437
Notes 442

Edited by the Sage School of Philosophy, Cornell University

THE PHILOSOPHICAL REVIEW

A Quarterly Journal

VOLUME LXV, NUMBER 3, WHOLE NUMBER 375

EDITED BY the Faculty of the Sage School of Philosophy in Cornell University: Rogers Albritton, Max Black, Stuart M. Brown, Jr., E. A. Burtt, G. Watts Cunningham (Emeritus), Richard G. Henson, John H. Hick, Norman Malcolm, John Rawls, George H. Sabine (Emeritus), David Sachs, and Harold R. Smart.

EDITORIAL BOARD

Stuart M. Brown, Jr., Norman Malcolm, John Rawls

Articles intended for publication, books for review, and subscriptions should be sent to THE PHILOSOPHICAL REVIEW, 231 Goldwin Smith Hall, Cornell University, Ithaca, New York. Subscription rates: $3.00 per year to individuals; $6.00 per year to institutions; single copies $1.50. The articles in this REVIEW are indexed in the INTERNATIONAL INDEX TO PERIODICALS, New York, New York.

Second-class mail privileges authorized at Ithaca, New York.

PRINTED BY [illegible], ITHACA, NY, THE [illegible] PRESS LTD.

CONDITIONS OF OBSERVATION AND STATES OF OBSERVERS

I

NORMALLY when we talk about perception and about what is perceived, we use material-object language. That is, we talk as if we could and did perceive—see, touch, hear, taste, smell—external physical objects which constitute our evironment. At the same time we admit that sometimes we are liable to perceptual errors and to hallucinations. For instance, under sodium lighting we see as black what is in fact red; from an angle of twenty degrees we see as elliptical what is in fact round; we can also sometimes confuse a nonperceptual hallucinatory experience *as of* seeing a snake with a perceptual experience *of* seeing a snake, and so forth. We explain the occurrence of a perceptual error or an hallucination by saying either that the conditions of observation, on that occasion, were not normal, or that we, the observers, were not in a normal physical and/or psychological state, or that we were simply careless and inattentive.

Now I believe the phrases "normal conditions" and "normal state of the observer," as thus used in such an explanation, are by no means clear and easily understood. On the contrary, they have given rise to many puzzles and confusions and in that way have contributed their share toward more than one mistaken theory about perception. In the following pages I want to do something by way of clarifying them. In particular I want to get an answer to the question "What is it to say that one, and not another, set of conditions—one, and not another, state of the observer—is the normal, or the standard, or the better one for observing certain features of our environment?"

II

I shall first outline and then criticize the not altogether uncommon view that saying of conditions X that they are, and of conditions Y that they are not, the standard or normal ones for

observing a characteristic ϕ is merely following a well-established but in its nature quite arbitrary convention. This view can best be put in terms of *the shape* of flat objects.

We can imagine someone saying: "So far as observation of the shape of flat objects goes, there is no one set of conditions which would be distinguishable from other sets of conditions by specific objective characteristics—characteristics such that we could point to them and say, 'These are the ones that make it possible to observe directly the real shape of, for example, a penny,' or in another case, 'These are the ones which make it impossible to observe directly the real shape of the penny; they make the penny appear the shape that it is not.' There are no such objective characteristics; there is only a convention to the effect that we shall say, 'This is round' whenever there is something that *is seen as round from straight-on.* That convention could have been different; we could have been taught to say, 'This is round' of anything that is seen as round from an angle, say, of twenty degrees to its main surface. Therefore there is nothing but an arbitrary convention behind the fact that we use the expressions 'normal (or standard) conditions for observing shapes of flat objects' and 'X is ϕ-shaped,' or 'The real shape of X is ϕ,' as we now do. And what is true of shape is equally true of other kinds of characteristics like color, magnitude, and so forth, as well as of the very existence of material objects. In all cases the distinction between real and apparent characteristics and between real and apparent existence, which we are required to make if we want to go on talking material-object language, must be based on mere convention because in all cases the distinction between normal and nonnormal conditions and states is so based."

Let us first be quite clear as to the sense in which the word "conventional" has been used here. For there is a sort of conventionality about our language which is quite universal and not in the least objectionable. For example, we could have used the word "green" as we now use the word "table." But of course, unless all green objects were suddenly annihilated, we should still have need for a symbol which functioned exactly as "green" now functions, both in its application and its entailments. Now this unremarkable sort of conventionality is not, and cannot be,

the sort that is attributed to the expressions "normal (or standard) conditions" and "the real shape of *X*." The claim is rather that we could assign different applications to these without thereby creating a need for new sets of symbols which *both* in entailment *and* in application function exactly as these expressions now do. And this would be the case because "standard conditions" and "the real shape of *X*" in their new application would retain their old entailments. To put the difficulty paradoxically: it looks as if their entailments were not tied down to the way they are applied. Or to put it in yet another way: their connotation does not seem to determine their denotation. It is felt that we have to determine the latter arbitrarily and, as it were, from outside. That is, we can lay it down first that by "the real shape of *X*" we shall mean that shape which is directly observed under standard conditions. And we can lay it down that by "standard conditions" we shall mean those conditions under which we can directly observe the real shape. But, as the reasoning goes, after having provided those expressions with just these entailments, we can toss a penny as to what we are going to do with them next, how we are going to apply them to the world. We can apply them as we please, so long as we adhere to the above entailment rules.

This, then, is the conventionalists' position as I understand it. I think it can be proved to be quite untenable if we turn from round pennies to oval plates, oblong table tops, and so forth. This will be done later on, but for the moment I want to bring out exactly how much plausibility there is in the position, provided we agree for argument's sake to concentrate our attention *solely* on such examples as pennies. Even then it is not true that we can decide *quite* arbitrarily whether we are going to say, for example, that the shape of pennies is really round—and thereby rule that the standard conditions for observing them is from straight-on; or whether we are going to say that the shape of pennies is really elliptical—and thereby rule that the standard way of looking at them is from an angle of, say, twenty degrees. This becomes clear if we consider how we use such expressions as "round," "elliptical," "from straight-on," and "at an angle of twenty degrees."

To take "round." Wittingly or unwittingly we all apply "round" to things whose shape approximately exemplifies the

formula of a Euclidean circle. And if we are challenged when we say, "This is round," then the correct procedure to adopt is to show—if possible, by direct surface measurement—that whatever we have been talking about does, more or less closely, fit the formula of a circle. The same goes for "elliptical," "right angle," and "an angle of twenty degrees": we can say that when we ordinarily talk about shapes and angles of observation, we are in fact all the time doing applied Euclidean geometry in a very rudimentary manner.

But it is already determined in the Euclidean system of *pure* geometry that, as a whole, that system can be applied only from straight-on. For suppose we adopted the convention of doing our applied geometry from, say, twenty degrees, then how would we decide whether any given angle is one of twenty degrees or not? We would have to determine this from another angle of twenty degrees, and so on ad infinitum. Of course, we could make it a rule to do our applied geometry according to two methods: (a) we could determine from straight-on whether or not an angle fits a given formula, and then (b) proceed to investigate what other formulae fit the world from that angle. But it is quite clear that methods (a) and (b) would not be independent of each other. We would know how to apply method (b) only if we already knew how to apply method (a). Further, we could reduce (b) to (a) at any time we liked, but we could not reduce (a) to (b) without falling into nonsense. And this amounts to saying that Euclidean geometry does not admit of more than one *basic* method of application.

Of course, it is not proved thereby that if we want to talk about shapes at all we are *absolutely forced* to use the expressions "the real shape of X" and "standard conditions for observing shapes of flat objects" in the way we now do. But it is proved that, everything else remaining the same, their use is not a matter of arbitrary decision in a quite straightforward sense. If we want to change it we have to change all that layer of our ordinary language which has anything at all to do with geometry and its application. We have, among other things, to change the meaning of words like "round," "straight," "square," "angle," and so forth. In short, we have to take up in our language a system of geometry which is

radically different from the Euclidean one. And, keeping in mind the fact that all sorts of non-Euclidean geometries have already been developed and that many more could be developed if people set themselves on doing it, I think, we have to grant the possibility of a system which would be consistently applicable in a quite different way from the Euclidean one. But it is very important to remember that then again "real shape" and "standard conditions" would not be tied only to each other. They would be tied to the whole new system much in the same way that they are now tied to our present old Euclidean one.

Notice, now, that as soon as the real shape of pennies came up for discussion, we found it natural to talk about "standard," and not so much about "normal" or "good," conditions for its observation. This was not simply a personal preference. "Normal" carries with it the suggestion of usualness. And there is nothing unusual about observing pennies from all sorts of angles: we view them from one angle almost as frequently as from any other. Nor is it impossible, or even difficult, to find out, by looking from almost any angle, whether they are round or not. Only not all these would be *direct* ways of finding out. This has to be explained. When we view a penny from what we ordinarily call a non-standard, that is, oblique, angle, it usually looks to us the way round things do look from such an angle, and we have no difficulty in telling what its real shape is. But if we were aware only of the penny itself and not of at least a substantial part of the context—including the angle of observation—within which the penny is placed, for example, if we cut off the context completely by looking through a narrow tube, then probably we would be deceived into thinking that it is really elliptical. And this means that the way the penny looks from such an angle, taken by itself, is deceptive and that only the awareness of the context saves us from being deceived. Now if we said that it is *always* the context which saves us from being deceived by the "looks" of the penny, then we would have, in effect, turned the penny into a thing-in-itself and committed ourselves to a representative theory of perception, because we would be saying that all we get from the penny are mere appearances. But there are simply no grounds whatever for saying this. There are all the grounds for saying,

in conformity with ordinary language, that the penny, including its real shape, is directly observable, that is, *that one of the possible ways in which it may look is in no sense deceptive.* We can say that sometimes we see it *as it really is* and that on such occasions there is no need for us to turn to contextual matters in order to avoid falling into error. And these strictly nondeceptive occasions are the ones on which the penny is observed under *standard* conditions. This explains the use of "standard" here. The standard conditions are no more common than most others; nor are they much better or more convenient than most others for telling the real shape of the penny; they are distinguishable from the others only by the fact that when they prevail we are not liable to mistakes if we say "X is ϕ-shaped" solely on the grounds of its looking ϕ-shaped, or being seen as ϕ-shaped, while on all other occasions we would be liable to mistakes if we did this and completely disregarded the context. This explains what was meant by saying that under non-standard conditions the shape of a penny cannot be observed directly and that under standard conditions it can.

III

We have selected on purpose a kind of characteristic—the shape that objects may be said to have—about which conventionalistic-minded philosophers at least appear to be partially right. But so far our attention has been concentrated on one specific case—on what are ordinarily called flat, round objects. And we shall see that this appearance of being right vanishes as soon as we take into account along with round pennies also square and oblong table tops, rhomboid carpets, oval plates, and straight and bent sticks.

The things that can be said about any one of these latter can be equally well said about them all, including the notorious half-immersed sticks; therefore we can afford to base the following discussion on one example only. Take an oblong table top. If you walk around it with your eyes fixed on it at an angle of twenty degrees, its shape appears to change in accordance with the laws of perspective. Now if you want to adopt the convention of saying that "This is oblong" means "This is seen as oblong from

an angle of twenty degrees," you will have to say not that its shape *appears* to change, but that it actually *does change* when you thus walk around it. And since the only thing with which this change of shape is correlated is the change in the position of the observer, you will have to say further that the change in the position of the observer is, in some sense, *the cause* of the change of shape. But note that this is not an ordinary discovery of a new kind of cause. For instance, it cannot be compared with the discovery which, presumably, the cave men made: that material objects can be caused to change their shape not only by manipulating them with one's hands, but also—even more easily—by applying all sorts of tools. There is no similarity here: present-day philosophers did not discover in this same way that things can be caused to change their shape not only by using one's hands, as well as tools and machinery of all sorts, on them, but also—even more easily—by a change in the position from which they are observed. In other words, there is no similarity between *discovering* a new set of causal laws and *elevating* the well-known laws of perspective to the status of causal laws. In the first case there is an acquisition of some additional knowledge about material objects, that is, that they are such that it is *true* to say of them that they can be caused to change their shapes by the use of tools; but there is no change in the use of the phrase "material object." On the other hand, in the second case there is no additional knowledge of any kind about what have hitherto been known as material objects; there is only a linguistic change in the phrase "material object," that is, the new provison that it shall be used so as to make it *proper* to say of whatever it applies to that it could be caused to change its shape by changes in the observer's point of view—that it shall be used in the same way as now the phrase "sense datum" is being used. For it is part of the ordinary meaning of "material object" that whatever it applies to cannot be said to be affected by changes in perspective; and it seems to be part of the meaning of the technical term "sense datum" that whatever it applies to can be said to be so affected. Thus *promotion of the laws of perspective to the status of causal laws logically necessitates demotion of material objects to the status of sense data.*

But since the adoption of the new rule of saying that "This is

ϕ-shaped" means "This is seen as ϕ-shaped from an angle of twenty degrees" necessitates the promotion of laws of perspective to the status of causal laws, and since this, in its turn, necessitates exchanging material-object talk for sense-datum talk, the conclusion is that it is logically impossible to fit the proposed new convention within the framework of a material-object language.

There is another consideration which leads to the same conclusion: The two statements (1) "This table is parallelogram-shaped," and (2) "This table is not parallelogram- but trapezoid-shaped" would both be true simultaneously if the new convention had been adopted—provided they were asserted by two observers, both of whom looked (as we now say) at the same table at an angle of twenty degrees, but each from a different side. But if they are both *true*, they cannot both be about one and the same material object, since they are contradictories: for irrespective of whether the law of noncontradiction must necessarily form the basis of any kind of significant talk or not, there is no doubt that it is basic in our material-object talk. But there are no two material objects for these two statements to be about, so at least one of them must be about something else. But both of them have the same subject term. So if one of them is not about the material object—table—then neither is the other. We can find out what they *are* about by noticing how closely their logical behavior resembles the behavior which is attributed to the so-called sense-datum statements: If we substituted the expression "my sense datum of the (or as of a) table" for the expression "this table," which occurs in both of them, we would get two sentences of which every phenomenalist would say that both could very well be simultaneously true. So it must be the case that the word "table" in them, though it appears to be a material-object name, actually works as a sense-datum name.

The two foregoing considerations show that with regard to all other than round material objects there is no room whatever for choice as to the conditions under which their real shape can be observed. No matter with what system of geometry we worked we would still have to say of such objects that they are the shape they look from what we now call "straight-on." But this should not be taken as meaning that it is "the objective nature of reality,"

or something equally mystical, which absolutely forces us, or makes it logically necessary for us, to talk just in this one way. What makes it necessary for us to talk in this way is the logical grammar of the phrase "material object." If we talked differently —if we said that the real shape of objects is what can be directly seen from some other angle than that of ninety degrees—we would not be using the word "object" in the current sense of "material object," and we would not be talking our ordinary language any longer. For in our ordinary language the expression "material object," in its current sense, plays a very central part. Change the use of this expression and you get a quite different language—some variety of what in philosophy is known as sense-datum language.

So that if, for some reason or another, we have come to use material-object language, then it is no longer a matter of convention what we are going to call the real and what the apparent characteristics of those entities to which the expression "material object" is applicable; and closely connected with this, it is also no longer a matter of convention under what conditions each of their real characteristics can be directly observed. This is important because many people who are dissatisfied with our ordinary way of talking seem to think that we have no good grounds—apart from purely arbitrary convention, which could have been different even within the framework of a material-object language—for the distinction which we make between "real" and "apparent" characteristics. They seem to think, as we have seen in the previous section, that the adoption of a material-object language merely requires that *some* characteristics should be called "real" and some others "apparent," but that it does not lay down which ones; that this latter we have to decide arbitrarily—or, what is the same, that in making our decision we have to follow only an arbitrary convention. And they think that such a state of affairs is very unsatisfactory because we seem to be required to promote, for no reason whatever, some characteristics to the status of "real" and demote others, again for no reason whatever, to the status of "apparent." They prefer sense-datum language because it allows them to treat as equals what, in their opinion, undoubtedly *are* equals.

From the discussion of the real shape of pennies it appeared that there are some grounds for this dissatisfaction, though not all the grounds that are usually claimed. But now, after we have considered the shape of other than round objects too, it is becoming clearer and clearer that this dissatisfaction is quite groundless. Even round objects, if considered not in isolation but together with other flat things, do not present us with any problems. For if there is such a strict determination in all other cases than round, then it must also be determined what is to be the proper way of talking about round objects: the only proper thing to say about such objects as oval plates being that they are elliptical, it must be simply and obviously wrong to say of objects which are other than oval—such as pennies—that they are *also* really elliptical. And by the same token it must be simply wrong to say that they are really that shape which they appear from an angle of twenty degrees, or for that matter from any other than a ninety-degree angle.

By now we are in the position to provide an answer to the question "What is it to say, with regard to flat objects, that conditions X are, and conditions Y are not, the standard ones for observing their shapes?" It is to say that conditions X are, and conditions Y are not, those *implied or determined by the use of material-object language.*

This answer, when slightly adjusted, should hold good not only of flat surfaces but also of the notorious bent sticks. As for the shape of solids, the difficulty about standard conditions with regard to them does not arise in the same acute form as with regard to flat objects; sometimes, as with spheres, it does not arise at all. But, I think, in so far as it does arise, the same answer could be obtained, because in a sense a solid can be said to be the product of its various surfaces, or of the various sections of its surface, and when the shape of these sections has been determined, then thereby something has been done toward determining the shape of the solid.

IV

Let us now turn to another kind of characteristic—to color. Note again that the phrase "normal conditions for observing the

color of material objects" may be used to suggest more than one thing. It may mean (a) the most common conditions under which color observation is made; or it may mean (b) the conditions which are the best, or at least which are good enough, for discerning the color, which the thing in question really has, from all other colors.

In the first case, that is if "normal" is used in the sense of "most common," it is true to say that ordinary daylight is *the* normal condition for observing colors—in the same way as it is, in this sense, *the* normal condition for observing almost all other kinds of characteristics. And it is also true to say—as, I believe, conventionalist philosophers do—that it is more or less accidental that we happen to live under the sun. We might have been, for instance, the inmates of a Platonic cave, in which case the light of a huge fire would be the most common condition for color observation; or again, we might have been the prisoners of a powerful magician who chose to keep us all the time under lamps filled with sodium vapor. Thus, although we ourselves have not selected sunlight as a condition for most of our observations, yet it has been "selected" for us by causes independent of us; and this "selection" could have been different. This much is quite correct, but one is immediately making a mistake if one proceeds further to argue that, given that the "selection" of the permanent light conditions had been different, we would *properly* and *truly* say that the real color of a large number of objects was different from what we now *properly* and *truly* report it to be; and that therefore it is merely a matter of a cosmic accident that we now call this and not another "the real color of *X*." For the purposes of certain theories this would be almost as good as saying that the application of the expression "the real color of *X*" is conventional; still it is a mistake. It rests on a confusion of "normal" in the sense of "most common" with "normal" in the sense of "best" or "at least good enough." "Normal" in the first of these senses has nothing whatever to do with our judgment that this and not that is the real color of *X*, while "normal" in the second sense has everything to do with it. If, for instance, we want to tell whether an object is black or red, any conditions—not only ordinary daylight—under which we can discern these two colors and

distinguish them from each other will be said to be good enough for that purpose. And what is true of red and black is also true of any other color, as well as of any shade of color. Those conditions under which any given color can be discerned most easily are the best conditions for its observation; while those under which it can be discerned only with difficulty are more or less bad, but those under which it cannot be discerned cannot properly speaking be said to be conditions *for* its observation at all; they are precisely the conditions which *prevent* its observation. Thus sodium lighting constitutes neither part of good nor part of bad conditions for observing the real color of red objects because under it red cannot be distinguished from black. So any conditions with sodium lighting as one component are simply out of the question as conditions for observing red. And they would be out of the question even if we had to spend our whole lives under sodium lamps instead of under the sun. No doubt we would then think that those objects which we now truly describe as red were in fact black, but we would be mistaken in thinking thus, for we would still be talking in the same way as we now do, that is, of external objects *being colored*. But if we say that *colors are there in the things to be discerned*, then it is almost a tautological addition to say that those conditions under which we can *consistently* discern distinctions between colors and shades of colors which we cannot discern under other conditions are the proper conditions for discerning these distinctions; and it is also almost a tautological addition that of two sets of conditions, both of which enable us to discern the distinction in question, the better set is the one which enables us to discern it more easily.

It is clear, then, that however commonly a given set of conditions prevailed, it still might not be the proper set for observing the true color of some objects, even though it might be the proper set for observing the color of some others. And this applies not only to sodium lighting but to sunlight as well. It is conceivable that some of our ordinary objects have shades of color which, in sunlight, cannot be distinguished from some other shades of the same color. Maybe there are, for instance, shades of green which cannot be discerned at all under sunlight just as red cannot be discerned under sodium light. This is just conceivable; there are

no grounds for thinking that it might actually be the case. Still, if under a special artificial light an object which in sunlight looked to be uniformly green were found to be of two shades of green, we would have to say that sunlight cannot form part of the proper conditions for discerning these two shades and for distinguishing them from each other—unless, of course, we stopped saying that colors are there in the objects to be discerned, unless, that is, we stopped asking: "What is the color of *X*?" But so long as we do not stop asking this question, there is no element of decision, there is nothing accidental in our saying that one and not another set of conditions is the best for observing the true color of such and such an object. This is completely determined by the above linguistic usage and by our empirical knowledge about the discernibility of colors.

It is, however, conceivable that the empirical facts which we know about colors could have been different in a number of ways. And two of these ways deserve special notice. First of all, we can conceive of a new kind of light being discovered, under which every object appears to have a strange new color totally different from that which it is seen to be in ordinary daylight. And we can conceive of the total set of these new colors having a one-one correlation with our old "daylight" ones—for example, our red turning, under this light, into a color which we shall designate by "*a*," our green into a color designated by "*b*," our light green into "*b* —," dark green into "*b* +," and so on, color for color, shade for shade. We cannot foretell with certainty what we should say if this really happened—how we should change our linguistic habits to accommodate this strange case. But we may hazard a guess that probably the habit of saying that *things* are colored would be discontinued and some other mode of speech adopted instead. For instance, we might come to say, if we still wanted to go on talking material-object language, that colors are always really "*only* in the light" and that objects merely appear to be colored because their surfaces reflect some colors and absorb others. The above imaginary discovery, if it were actually made, would probably be regarded as an *empirical* proof that it is so.[1]

[1] Those scientists who talk about absorption of certain light waves and reflection of certain others are not making empirical but theoretical statements

But it is not likely that such a discovery will ever be made. And even if it were made it would not tend to show that our attributing of color to objects has rested on a mere convention; it would rather show that we have been mistaken in doing so, that our empirical knowledge has been insufficient.

A second way in which the empirical facts which we know about colors could have been different is the following: we might have noticed that things change color when put under different lights. We already have some plants whose leaves change from dark green to pale yellow if they are kept for some time in a comparatively dark place. Now suppose that there were plants which, if red light were shone on them, became gradually red and retained this color for a while, even after being brought out into the sunlight. Then they gradually faded out and became white like this sheet of paper. And under green light they became green, under blue light, blue, and so on. Suppose everything were like this—tables, chairs, and pennies included; and we noticed one day that objects started acquiring and losing their color more and more rapidly; we noticed that we had to take the flower out into the sunlight very, very quickly in order to see that it had become red under the red lamp. And at the end only champion runners occasionally succeeded in bringing it out with its color still on, until even they could not do it any more.

If this happened, I think we would say that changes in light *caused* objects to change their color. Most likely we would also say that *any* color which an object happened to have at any one moment was its *real* color and that there were no apparent colors at all. But again, these changes would not in any way support the view that it is a matter of convention whether we say, for example, that under a red lamp objects really *become* instantaneously red, or that they merely *appear* to be red. It would rather support the view that it is a matter of empirical observation which of these two we have to say. Without empirical observations similar to the ones described above, a change from saying that objects appear red under red light to saying that they become red would amount to changing from material-object language to sense-datum

about color. Their theories are not very unlike alternative languages—ways of talking different from our ordinary one.

language. For by arbitrarily elevating the laws of optics to the status of causal laws, we would achieve exactly the same with regard to colors that we achieved with regard to shapes by a similar elevation of the laws of perspective—instead of material objects we would be talking about sense data. And this we do not want to do.

V

Another consideration which is sometimes supposed to prove the conventionality of conditions of observation is that of the use of microscopes.

"With the naked eye one can see the grain of the table, but otherwise the table looks smooth and even. If we looked at it through a microscope, we should see roughnesses and hills and valleys, and all sorts of differences that are imperceptible to the naked eye. Which of these is the 'real' table?"[2] Which are the normal conditions for observing the real table? And the answer is supposed to be: "Whichever you arbitrarily select."

But mere attention to the use that is made of microscopes will make us realize that this argument is mistaken: microscopes, as everyone knows, are used for *magnifying* small objects. So let us for argument's sake suppose that with a naked eye we cannot detect, on a surface, any unevenness of less than .001 mm., and let us imagine that we have two microscopes—*A* and *B*—, and that with *A* we can tell with precision whether there is an unevenness, and how big it is, up to .0001 mm., while with *B* we can do this up to .00001 mm. In such a case we can imagine ourselves making the following three statements: (a) This table top is smooth within .001 mm. (that is, any unevenness which it may have will be less than .001 mm.); (b) This table top is uneven, and the unevenness is not less than .00050 mm. and not greater than .00059 mm.; (c) The unevenness of this table top is not less than .000530 and not greater than .000539. These statements, however badly they may have been chosen from the point of view of existing measuring techniques, are not contradictories of each other. Therefore they can all be asserted about one and the same

[2] Bertrand Russell, *The Problems of Philosophy* (Home University Library; London, 1912), p. 10.

table, and there is no occasion for asking which of them is about the *real* table. The only difference between them is that they describe the table progressively with more and more exactness. And further, the only difference between them on the one hand, and our everyday statements such as "This table is smooth to the naked eye" and "When looked at through the microscope this table appears to have hills and valleys" on the other hand, is that the former are more precise and that the fact that microscopes magnify is in them explicitly recognized. For by our everyday assertion "This table is smooth" we do not mean that it is absolutely smooth; we only mean that it is not within the powers of a naked eye to discern any unevenness on its surface. If we add to it a statement about the powers which our unassisted eyes actually have, we get the above more precise statement that the table is smooth within .001 mm. Similarly, by the statement "When looked at through the microscope this table appears to have hills and valleys" it is not meant that the table top is, or might possibly be, like a moderately undulating country; what is meant is that it looks like that *when magnified, say, a thousand times.*

Now I think some people who have felt that there are difficulties about microscopes might be satisfied with this and agree that both naked-eye observation and observation through a microscope is observation of the real table top and of its real characteristics, the only difference between the two ways being that the microscope magnifies a selected small section of the surface and thus allows us to observe it in more minute detail. They would agree, that is, that both naked eye and microscope are, in a sense, *conditions for* observing the real characteristics of an object. But it is also quite clear that others would be very far from satisfied with this. The only effect that the foregoing discussion would have on them would be to enable, or even to force, them to put their difficulty in a concise and rather clear manner. "Why should we say," they might ask, "that the microscope magnifies rather than that the eye *minimizes*? Why, for example, should we not say that the unevenness of the table top is of such dimensions as seen through the microscope or as seen by the mite?"

But as soon as they have put it in this way it is clear that they are under a misconception about the logical grammar of words

used for describing magnitudes, or, which is the same, under a misconception about the kind of characteristic that magnitude is. They take it to be something absolute when it is, in fact, relational. That is, they do not realize that describing the magnitude of something is essentially comparing it with something else, for example, comparing the mite's foot with a measuring rod; and that therefore, if the thing compared and what it is compared with are both magnified or minimized to an equal extent, the relation between them is not thereby changed—it is only made more or less exactly observable respectively. Even to say that they have both been magnified to an equal extent involves a comparison—a comparison between the two of them on the one hand and something else which has remained constant on the other. If the possibility of making the latter comparison is removed, then a sentence which purports to assert that something has been magnified, or even has expanded, comes to mean something quite different from what we would normally take it to mean. For instance, the sentence "*Everything*, including distances between objects, looks ten times larger to the mite than to us" comes to the same as "The eyes of a mite are more narrowly focused and ten times sharper than ours." Equally, the sentence "The Universe and everything in it—including measuring rods—has become ten times bigger during the night" comes to the same as "People's eyesight is ten times sharper than it was yesterday." And the question "Is *everything* as big as it looks through the microscope or as big as it looks to the naked eye?" comes to "Are our eyes as narrowly focused and as sharp as microscopes?"; which latter is a quite straightforward empirical question to which the answer is a confident "No."

When this is understood, the difficulty about *everything* being either just as big as, or bigger or smaller than, we see it vanishes completely. And we are left with only the question "But is *this particular object* as big as it is seen by the naked eye or as big as it is seen under the microscope?" which also, when looked at more closely, is found to have an unexpected meaning. It does not mean "Which are the correct measurements of this object—the ones obtained with, or without, the help of a microscope?" For if it meant that, then there would be no problem: we have already

seen that both measurements, if fully expressed, are compatible. What that question really means can be found out if you counter it by another question, "And how big is it seen under the microscope?" It is most likely that then your attention will be drawn to the distance between the two pointer fingers of the other person. "That big," he will say. But since you can proceed to measure this distance, he could just as well have said, for example, "It looks about 1.3 cm. long and 1.1 cm. wide," from which you can see that his *technique of comparison*, when making magnitude judgments, is rather peculiar: he looks through the microscope at some of the things which enter into the comparison and at others just with an unassisted eye. This makes it clear that his question, "But is this particular object as big as it looks to the naked eye or as big as it looks under the microscope?" is really a question about comparing techniques—"Can we *directly* compare an object which is under the microscope for its real magnitude with another object, or objects, which is, or are, not?" And now the answer is quite easy: "You cannot do that if the object in question is a material object and not a sense datum." For doing that would amount to the admission that material objects change their magnitude when put under the microscope—that looking at them through the microscope is the *cause of* their becoming bigger; which would be an admission *of the same kind* as the one that objects change size with distance of observation. And we have already seen what is involved in elevating laws of perspective to the status of causal laws: material-object language is exchanged for sense-datum language. And that is not what we want to do.

So the upshot of the present section is this: either the person who asks the question "Is *X* the size that it appears to the naked eye or the size that it appears under microscope?" is confused about the way we talk about magnitudes and thinks that we talk about them as a kind of absolute characteristic, or else he correctly recognizes that our talk about them is based on comparisons. If the second, then again either he wants to talk material-object language or sense-datum language. If the first, then he must keep under microscope *all* the objects which he wants to compare *directly* or else he must dispense with the microscope altogether. Otherwise he will not be comparing material objects

but sense data. And it may be noted that exactly the same reasoning holds good for the question "Are things as big as they are seen from close-up or as big as they are seen from some distance away?" So again we see that when one has chosen material-object language, there is no further choice left for him as to the conditions of observation.

VI

By now we have some idea of what it is to say that conditions of observation are either good, or usual, or standard for observing shape, color, and texture of material objects. There are other *kinds* of characteristics besides these three, for example, temperature, hardness, and so on, but we need not go on and discuss them all, for it is unlikely that they would confront us with difficulties of a different sort from those which we have encountered already. Let us turn instead to the physical and psychological states of the observer and ask: "What is it to say that they are normal?"

Whether, in a perceptual context, a person can be said to have, for example, a normal, or else defective, eyesight and be in a normal, or else abnormal, psychological state, depends on the kind of characteristic and the kind of object with which he has to deal. Thus for telling whether there is a red or a green rug on the floor it is enough if we are not either red-green color-blind or liable to hallucinations; it does not matter at all that we need + 5 glasses and have not got them on at the moment. On the other hand, it does not matter whether we are color-blind or not if what we have to tell is whether the carpet is square or oval, or whether there is a snake on it. What is needed here is that we should not be terrifically shortsighted or suffering from hallucinations. And in this manner, if we cared to go on, we would be able to list what count as queer or defective states of the observer in many more cases.

When the normality of a given observer is established separately with regard to each of the more common kinds of characteristic, such as color, shape, and heat, and with regard to the very existence of material objects, then we can make a generalized statement as to his over-all normality as a percipient, for example, that he is neither color-blind, nor terrifically shortsighted, nor

insensitive to heat, nor prone to hallucinations—that is, that he is capable of perceiving the objects which happen to constitute his environment the way they really are.

There are no philosophical difficulties in this final step: in the assertion of a conjunction instead of a number of simple statements. All the difficulties are connected with the simple statements like "Smith is not color-blind" and "Jones does not suffer from hallucinations." And to these difficulties we must now turn.

We must ask, for example: "What is it to say that Smith is not color-blind?" And a well-known answer to this is: "To say that Smith is not color-blind is the same as to say that Smith's reports about the color that objects have are in agreement with the reports of the majority." And since the man with the normal color vision sees the real color of things, according to this answer "The real color of X is red" must mean the same as "X is seen as red by the majority."

This opens the way for the following argument: "But it could have been the case that most people had such visual experiences as, for instance, are now had by those whom we, at present, call 'red-green color-blind.' In such a case we would correctly report that X was in fact green and not—as we now do—that its real color is red. But this means that when on any occasion we are reporting the real color of a given object, we are actually saying, in a concealed and misleading way, something about the color sensations which *we* have and which most other people would have if they happened to be in the situation in which we are at the moment—if you like, we are saying that we have, and that most people would have in this situation, red sense data and not green ones."

Such a conclusion is very much in agreement with phenomenalistic doctrines and very much against the way we ordinarily think and talk about colors. For undoubtedly we think of them not as sensations of the observer but as characteristics of physical objects. And what the above argument does is show that our thinking about them in this way is quite unwarranted.

But if we think of them in that way, then the very first step of this argument should be suspect: how could one say that "Smith is not color-blind" means the same as "Smith's reports

about the color that objects have are in agreement with the reports of most other people"? Surely the correct thing to say then would be that "Smith is not color-blind" means "Smith sees the real color of objects." But of course this would not get us very far; we can imagine it being said by way of an answer: "Smith sees the real color" in its turn just means "Smith's reports are in agreement with the reports of the majority." So to dispose of the phenomenalists' argument we have to deny, at some stage or another, that ordinary perceptual statements are equivalent to statements about agreement between people's sensations. And this is not so hard to do as it might at first appear. Doing it by no means involves a departure from ordinary language; rather, it accords with it:

For easier understanding let us consider a case as simple as possible. Let us imagine the world's total population reduced to one hundred persons, all of whom live together on an island where there happens to be a large paint store with all the paint unlabeled. Now suppose one day, for some reason or another, they decide to put labels on all the paint tins, and in the course of doing so disagreement arises among them. One man says that the rest of them are hopelessly messing up the job by putting the same label "green" on paints of two quite different colors. In accordance with the phenomenalists' principle we would have to say that since this man's report is inconsistent with the reports of the overwhelming majority, he must be wrong, that is, that he must be in such a physical or psychological state that it is impossible for him to see that paint as it really is. But I do not think that this is what we would do. It seems to me that it would be more proper to do the following: let him sort out our "greens" in the way he likes, then send him away, mark the tin bottoms of his one lot with the letter "G" and of his other lot with the letter "R," then mix the two lots together again, call him back, and ask him to separate them out by looking merely at the color of the paint and not at our marks on the bottom of the tins. If he does it again without making one single mistake—the tins themselves being all identical in size and shape—then it would be very odd to go on insisting that his eyes are defective and that ours are all right. We might, of course, still think that he is playing some

subtle trick on us; we might want to test his ability still further, for example, by letting him pick out bits of wood, some of which we have painted with paint from one of his stacks and others with paint from the other stack, and so on. But sooner or later, I think, we should come to believe that what he says is true—that there are some quite clear differences in the color of the paints which we are not able to see. And we should admit that his eyes are all right for perceiving that difference while ours are not. This case shows that the sentence "Smith is not color-blind," as it is ordinarily used, does not mean the same as "Smith's reports about colors tally with the reports of most people."

But it does not show that agreement between such reports has nothing to do with our judgments about Smith's or anybody else's normality as a percipient. To prevent further misunderstandings, we have to find out what exactly agreement has to do with it. For this purpose consider, by way of comparison, the sentence: "If I have added up this column of figures correctly, then if other people who know mathematics added it up, they would also get the same result." We are much less tempted to say here, in the case of mathematics, that to get the right result is to get a result which everyone else is getting and to get it repeatedly. It is just possible that ten thousand people are all of them wrong in a particular addition and that Smith alone is right. But though possible it is very unlikely, and it is still more unlikely that they, working independently, could all get the *same* wrong result. And just because of this unlikelihood they can be used as checks on Smith. But this unlikelihood is a mere contingent fact. The world might have been so constituted that if Smith made a mistake in calculation, then everybody else living in that town would be on that day liable to make the same mistake in the addition of the same row of figures. If this happened, we would do two things, I suppose: we would set up some sort of theory about mental interference, and we would send out our calculations for checking to faraway places. But we would not say that, for example, $2 + 5 + 7 + 3 + 4 = 20$ *because* that is what all people in Melbourne get today when they add up this row of numbers. Nor would we say this if the mental interference affected the whole world and made everybody miscalculate for the next six months

or so. We would only be extremely careful in doing our sums, and, maybe, the parliament would pass a law to the effect that everybody caught in careless mathematical practices was to be confined to jail for so many days.

So getting the *right* result is not the same as getting the *same* result. But as a matter of contingent fact, the only method of checking whether we have got the right result is to see whether it tallies with everybody else's.

Now I submit that we can say very similar things about the sentence "If this object is actually red, then most other people will also see it as red" and equally—to bring in hallucinations—about the sentence "If that is a real dagger which I see, then other people will see it too, and I myself shall be able to grab it at its hilt." For in the same way that we would be surprised if a number of people had got the *same* wrong result in calculation, so also would we be surprised if a number of people were affected in such a way that they saw objects in the wrong colors and yet their descriptions of them tallied. And we would be even more surprised if they had hallucinatory experiences, for example, as of a snake on the carpet, which would be perfectly consistent. It is easy to explain how we get the same results in calculation: we all work according to the same rules. And it is easy to explain how our reports about our surroundings are consistent with each other's: we all see the same things and the same characteristics of these things. But if we did not work with the same rules and if we did not see the same things and the same characteristics, we would be forced to say that we get the same results and give the same reports merely by a sheer long-odds coincidence. And this fact constitutes a sufficient reason for using agreement as a test for finding out whether we are in a condition to see the world as it really is. There is no need for any further reasons in support of the validity of agreement—neither in its perceptual nor in its mathematical application. In particular, there is no need to insist that if this test is to be valid then it must be the case that "seeing the world as it really is" *means* the same as "getting compatible results by all our senses *and* getting the same reports from the majority of observers." This is not required for the *validity* of the test; it would be required only if we wanted to make the test *infallible*.

But we do not, ordinarily, regard it as infallible; we admit the possibility that on some rare occasions, relying on this test, we *might* go wrong; as, for example, in the color case if the world's population had been reduced to ninety-nine instead of a hundred, all of them color-blind.

Now we have reached a stage when there is nothing in the way of understanding that again material-object language leaves us no choice, no room for an arbitrary convention, as to what we are going to call "normal" and what "abnormal" states of observers. This is determined by the fact that when using such a language, we talk of things as existing objectively in physical space and having characteristics quite independent of whether they are observed correctly or incorrectly, or whether they are observed at all. For if we talk that way, then we must also say that the state of an observer is normal in so far as he can observe the features of the external world, each of them, correctly; and that to the extent to which he misperceives some of them, or is liable to hallucinations, his state is abnormal. We must say this and we cannot say anything else. In particular, we cannot say that "normal" here just means "usually makes the same perceptual reports as most other people."

VII

Let us be clear, in conclusion, about what I have and have not attempted to establish in this discussion. I have *not* attempted to establish that it is not a matter of choice or convention whether we talk material-object language or some other—for instance, sense-datum—language. Maybe those philosophers who say that sense-datum language is a logical possibility are right, maybe they are wrong. Nothing has been said about that here. Nor has it been said that, provided they were right, and provided sense-datum language were not only logically but also "humanly" possible, there would still be good grounds for sticking to our ordinary material-object language. What I have tried to establish is this: given that we have come to talk material-object language—for some reason or another, or for no reason at all—*then* we can no longer choose, *then* it is no longer a matter of convention, as to

what we are going to call real objects and what mere appearances of them and as to what we are going to call real and what merely apparent characteristics of these objects; nor is it *then* a matter of convention what we are going to call the normal, the standard, the best, conditions for observing them and their real properties; all this has been determined once and for all by the fact that we have adopted material-object language. And this has some importance because in the history of philosophy we find a large number of arguments—not infrequently repeated by contemporaries—which start out, in effect, by giving reasons for saying that in material-object language the distinction between "normal" and "other than normal" conditions of observation is quite arbitrary and that therefore so must be the application of "real" and "apparent" to what is *directly* observed, and which then proceed to say that if there is something real in a nonarbitrary sense, it must be unobservable to us—that at least it cannot be directly observable in the way in which what we now misleadingly call real objects and real characteristics are directly observable. But as soon as we come to understand that *in* material-object language we *could not* use the expressions "real," "apparent," and "normal conditions," "good conditions," "queer conditions," and so on, in a different way from that in which we now use them, the urge to look for some ultimate, nonarbitrary reality beyond what is observable disappears. And with it will disappear the urge to go in the opposite direction and contend that there is no sense in talking about anything else apart from our "experiences" or sense data and that both what we call "real objects" and what we call "appearances" are, in fact, logical constructions out of these sense data—the difference between the two consisting in the order in which the data or experiences follow each other.

R. ZIEDINS

University of Melbourne

A CRITICISM OF PLATO'S *CRATYLUS*

IN AN article in the *Revue Internationale de Philosophie*, 1955, I have described the nature-theory of names which Plato made his 'Socrates' develop in the *Cratylus*, the theory, namely, that "everything has a right name by nature" (383A). In the present article I first ask whether Plato himself believed the nature-theory of names and then criticize the theory.

Did Plato himself believe the nature-theory of names which he makes his 'Socrates' develop in the *Cratylus*? Perhaps we should ask first whether it is reasonable for us to venture any answer at all on this matter. Perhaps we ought to renounce trying to say whether Plato believed the nature-theory of names. Plato was a dramatist and thought of himself as such; and the business of a dramatist with ideas is to present them, not to judge them. And the dramatic play of ideas is more prominent in the *Cratylus* even than in the *Phaedo*. Only in the *Cratylus* does Plato's protagonist argue against each in turn of two contrarily disposed respondents. The *Cratylus* more than any other of Plato's dialogues except the *Parmenides* justifies Professor Robert's remark that Plato like Rabelais was a mystifier, "*le plus génial mystificateur de tous les temps*" (*Congrès Budé* on Plato and Rabelais [1953], p. 418). The *Cratylus* gives the impression of putting forward ideas for consideration without deciding which of them are true. To deposit ideas in the public domain and to examine them is what it represents 'Socrates' as proposing to do (384C7) and itself in effect does. It is easy to regard the dialogue as showing Plato in a difficulty, unable to say a confident yes or no to a certain proposition. Furthermore, the end of the dialogue is like a way of saying that we have not got to the bottom of the matter and that it needs further study. 'Cratylus' and 'Socrates' tell each other to keep on inquiring and to let each other know if they discover anything.

These considerations might well lead a reasonable man to suspend judgment permanently on the question whether Plato himself believed the nature-theory of names. On the other side,

there is some evidence suggesting an answer to the question, as follows:

The theory, that the right name for a thing is a natural revelation of the essence of that thing, is deeply attractive to many people. It explains the fact that we often feel as if the name had the same qualities as its nominate. The word "ugly" sounds ugly. The word "beautiful" sounds beautiful. The true name of a terrible god would be so terrible that it is far better not to inquire what it is. (See S. N. Behrman in *The Worcester Account.*) Pigs are rightly called "pigs". Civil servants demand their victims' "true names" and "full names", refusing to admit that a person's name is what he calls himself.

Plato shared this feeling that a thing has a true name and that the name has the same qualities as the thing. If one thing is more honorable than another, its name is also more honorable than the name of that other (*Phaedrus* 244D). Some names are terrible and fearful (*Republic* 387B). His 'Socrates' confesses to a more than human fear with regard to names of the gods (*Philebus* 12C). We have here, therefore, a small likelihood that Plato held the nature-theory of names.

On the other hand, it is possible to have this feeling about names and yet to disapprove of the feeling when one comes to make a serious investigation, just as one can be afraid of ghosts in the night in spite of being intellectually convinced that there are no ghosts. And the above evidence about Plato's *feelings* seems to be outweighed by the following evidence about his *views*.

In several dialogues Plato makes his protagonist express the view that it is possible to go astray in investigation by attaching oneself to the name instead of to the thing, and demand that the company shall attend to the thing rather than to its name. To contradict the name rather than the matter is not dialectic but quarreling (*Republic* 454A). "Our aim is not to say the name; it is to consider the thing named" (*Theaetetus* 177DE). "An easygoing attitude towards names and expressions, and a not examining them minutely, is not ignoble on most occasions; in fact, the opposite attitude is illiberal, though it is sometimes necessary" (*ibid.* 184C). "At present you and I have only the name in common with regard to this creature, and the thing to which we apply

the name is perhaps private to each of us; but we ought always to agree on the thing itself by means of logoi rather than merely on the name without a logos" (*Sophist* 218C). "Good, Socrates; and if you persevere in not taking names seriously, you will appear richer in wisdom as you approach old age" (*Politicus* 261E).

This detached attitude toward names has probably often been taken by people who also assumed that names have a natural connection with their nominates. It is, however, unreasonable to combine this attitude with this assumption; and those who reflect much on the nature-theory are likely to see the unreasonableness. Plato did reflect much on the nature-theory, for he wrote the *Cratylus*. His contempt for names is therefore some small evidence that he did not hold the nature-theory.

The view that there are by nature correct names for things is never asserted in any work of Plato's besides the *Cratylus*. But the contradictory view, that there is no natural connection between a name and its nominate, is clearly implied, and made the basis of an important argument, in *Letter VII*. "None of them has a firm name. What is now called curved could just as well be called straight, and what is now called straight could just as well be called curved, and these interchanged and contrary usages could be just as firm. And the same statement holds of statement, since it is composed of names and expressions: it is never sufficiently firmly firm" (*Letter VII*, 343B). This is a large part of the premiss from which Plato there concludes that if Dionysius had been a true Platonist he would not have written a work of Platonic philosophy. This passage does not use either the word "nature" or the word "agreement"; but it is hardly compatible with the nature-theory of names in the *Cratylus*, and a person who had written the *Cratylus* would probably notice that. *Letter VII* is also out of sympathy with the nature-theory when it complains of "the weakness of language" (343A1) and of "the bad nature" of names and statements (343D8).

Plato's greatest pupil laid it down from the beginning that "a name is a sound that is significant by convention. . . . I say by convention, because no name exists by nature. Nothing is a name until it becomes a symbol. Merely showing something does not

constitute a name. Wordless noises show something, for example the noises of beasts; but none of them is a name" (*De Interp.* 16a19-29).

The above evidence suggests that Plato believed that there is no natural rightness of names. The *Cratylus* itself suggests the same after we have critically examined and weighed the arguments which his 'Socrates' there produces. For the considerations which 'Socrates' finally brings forward against the nature-theory (434-439) are upon reflection undeniable truths. The passage is as adamantine an argument as you can find anywhere in Plato. It certainly is custom that enables us to understand each other when we do; and the power of custom certainly is wholly independent of whether the name we use resembles its nominate or not; and, however much names might resemble things, it certainly must be possible to learn about things otherwise than from their names; and it must be better to do so. I feel sure that every reasonable man who reflects on these matters to the extent of writing or studying the *Cratylus* becomes convinced of these propositions, and therefore that Plato became convinced of them.

But these propositions have an obvious bearing on the importance and interest of the nature-theory of names. They entail that this theory is rather unimportant and rather uninteresting at best. Whatever remnants of truth 'Socrates'' remarks may have left to it, he has at any rate completely destroyed its pretensions to provide a respectable method of getting to know the world, or even a respectable account of actual names. In fact, there seems to be nothing left of it but a vain regret: "Perhaps as far as possible it would be best if all or most of our names were like their nominates." (435C. Méridier seems mistaken in rendering '*ἴσως*' by "*sans doute*".) The agreement-theory is "vulgar" (435 C); but, 'Socrates' seems to imply, it is inevitable. Throughout the conversation he had refrained from adopting the nature-theory himself, rejecting 'Hermogenes'' imputation of it to him (391A cancels our natural inference from 390E1) and representing all his inventions as inspirations from Euthyphro.

I conclude with some confidence that, while Plato may have entertained the nature-theory favorably before he wrote the *Cratylus*, and even while writing it, he soon came to see that it had

no practical use, and he finally came to the convention-theory implied in his *Letter VII* and stated by Aristotle. His writing of the *Cratylus* may have been a sort of purgation of the nature-theory from his mind, whether or not he already expected it to be so when he made his 'Socrates' talk of purging the inspiration of Euthyphro (396E).

Someone may object, however, that if Plato on completing the *Cratylus* had rejected the nature-theory of names, he would have answered the arguments in favor of that theory which he makes his 'Socrates' develop in the early part of the dialogue. Let us consider this objection.

It cannot be a fatal objection, because Plato makes his 'Socrates' answer neither the arguments which he brings for the theory at the beginning of the dialogue nor the arguments which he brings against it at the end. The suggestion that Plato must have believed the nature-theory because he did not refute the arguments he developed for it is, therefore, neutralized by the suggestion that he must have disbelieved it because he did not refute the arguments he developed against it.

It is worth while, however, to examine the arguments for the nature-theory to see whether they are as strong as the arguments against it, and whether they would have appeared so to Plato. The first argument is that since statements have a truthvalue, their parts, including names, must have a truthvalue too. Therefore names are true or false (385BC). The argument appears to end here, without explicitly arriving at the nature-theory. But we seem to be expected to think that the view that there is a natural correctness of names follows from the view that names are true or false. In other words, the natural rightness of a name is its being true, and its natural wrongness is its being false.

This argument is bad; for names have no truthvalue, and the reason given for saying that they do is a fallacy of division. No one in the dialogue points out that it is bad; and 'Socrates' makes the converse mistake of composition later on (431B). Nevertheless it is fairly probable that Plato saw or at least felt that it is a bad argument, quite different in quality from those he later produces against the nature-theory. In his *Sophist* (263AB) he said that truthvalue is a character of statements and that statements divide

into names and predicates (*ὀνόματα καὶ ῥήματα*, 262C), and he did not say that names have a truth value.

The second argument is: Men differ in goodness and badness; therefore men differ in wisdom and folly; therefore things have a firm nature of their own independently of us; therefore actions have a firm nature of their own and can be done well only in the way that is natural to them; therefore speech can be done well only in the way that it is natural for things to be said; therefore naming must be done in the way it is natural for things to be named (386-387).

This is a vague argument rather than a bad one. There is something in it; but it is by no means clear precisely what, or whether what there is in it is precisely what is needed to establish the nature-theory. For example, is naming here using an existing name, or inventing a new name, or both? And again, what kind of nature is in question here? The "natural" way of speaking sounds suspiciously like the customary way of speaking, in which case this is not an argument for the nature-theory of names at all. This argument, therefore, though not without suggestiveness, has nothing like the undeniable quality of the final arguments against the nature-theory. Its element of truth seems more like an assertion of the custom-theory maintained by 'Socrates' at the end of the dialogue.

The third argument is this: the name is the instrument by which we inform each other and distinguish how things are; like all instruments it is given to us by its maker, in this case the lawgiver, and its making requires technical knowledge (388). The competent lawgiver makes a name by looking to "that itself which is name" and embodying this form in sounds and syllables; for that is the way in which all good tools must be made (389). And his work is directed by the appropriate overseer, namely the dialectician (390).

This argument is not so much an argument as a free development of the nature-theory on the assumption that a name is a tool like a shuttle. It contains no undeniable observations like those which attack the nature-theory at the end of the dialogue. It all rests on the easily deniable assumption that a name is a tool like a shuttle. There are tools which we have to make before we can

use them, including the shuttle. There are organs which we do not make but possess as parts of our physical equipment, including the hand. A name is rather more like a hand than a shuttle. But it is very different from both; for it belongs to that third realm, neither nature nor artifact but culture. It is acquired, like a shuttle, but unconstructed, like a hand. Plato, although he has been called a fine sociologist, probably saw little of this; but he probably did see that the argument is weak and fanciful and little more than a fairy tale, in contrast with the hard facts that he produces against the nature-theory at the end. He probably did see that the whole notion of the lawgiver and of the name as like a tool is unsuitable in view of that supremacy of custom in speech which he points out at the end. He probably felt that custom, which he failed to distinguish from agreement, by its role in language excluded the possibility of a name's being a tool like a shuttle, just as Aristotle implies that for a name to exist by agreement is for it not to be a tool (*De Interp.* 17a1).

I conclude that the appearance of Plato's distributing himself equally on both sides of the question is deceptive. In favor of the nature-theory he produces only arguments that are weak or bad; against it he produces unchallengeable arguments; and he probably felt this himself. This was the view of Wilamowitz.[1] George Grote, however, who was a perceptive and judicious interpreter of Plato, thought that Plato at the end of his *Cratylus* was still believing that names having natural rectitude were possible and desirable, though not actual.[2]

I pass now to some other comments on the nature-theory of names and on the manner in which it is discussed by the characters in the *Cratylus*.

The nature-theory of names expresses and confirms our desire to see ourselves as reasonable beings. To say that names are right is to say that they are reasonable and there is a reason why a thing has the name it has. There lies behind the nature-theory a tendency to believe that man has a reason for everything he does, or at least ought to have a reason for it. A sound ought not to be connected to a thing merely by being used by men to designate

[1] U. von Wilamowitz-Moellendorff, *Platon* (Berlin, 1948), Aufl. 4, S. 223.
[2] G. Grote, *Plato* (London, 1865), II, 543.

the thing. Men ought to use, in order to designate the thing, some sound that has some natural connection with the thing apart from their use of it. If there were no reason why a thing had the name it had, names would be arbitrary, irrational, pointless, and inexplicable. And if that were so, man, the user of names, would himself be arbitrary and irrational in his language. He would have no reason for the way he talked. Playwrights, when they cannot untie their plots, are tempted to bring in a god on a machine. And we, when we cannot explain a name, are tempted to put it down to the gods or to a foreign language or to "antiquity". But those are all nothing but fine excuses. We ought to give a reason (*λόγον διδόναι*, 425D-426A) for the name.

Thus the nature-theory of names was part of what we may vaguely call Greek rationalism, that is, of a hopeful view of the powers of human reason and the extent to which it really controls human action. It was part of a unique climate of opinion in which human reason seemed to be limited neither as previously, by mysterious gods and customs, nor as now, by mysterious customs and unconscious desires.

We can never again believe in reason as confidently as the Greek philosophers did. But we still try to hide from ourselves the accidental element in language, just as pathetically as Plato's characters do in his *Cratylus*. We still try to show "reasons" why a given thing should be named by a given name. We hate to make a sheerly new sound, even when we have a sheerly new thing to name, for to do so would expose the arbitrary and irrational element in language. Hence in making a new name we nearly always profess to be using an old description. Even in christening a child, we prefer to use an already existing name, and we laugh at or repudiate those innovators who invent entirely new names.

The speakers in the *Cratylus* tend to assume that names are either entirely reasonable or entirely arbitrary, and that they will be driven to the latter position if they yield the former. But the fact is that, although there is no natural correctness of names independent of man, there nevertheless is a correctness of names and of other aspects of speech. Some utterances are definitely correct, and some are definitely incorrect. The nature-theory

looks for this correctness in a wrong place; but it does not look for a nonexistent correctness. On the one hand, the name of a thing does not belong to it by nature; on the other hand, a name can be correct or incorrect. On the one hand, we scholars see that there is no natural connection between a thing and its name; on the other hand, people constantly ask us which name is correct, and they are right to do so.

I proceed to try to give a better account of the real correctness of a name. Many rules and standards and principles may bear on a single act, and so it may be correct or incorrect in many different ways; and this applies to any particular utterance of a name. But if we seek to realize those aspects of its correctness or incorrectness which belong to it in virtue of its being a name, we must find its purpose as a name. The goodness or badness of a name and the rightness or wrongness of a use of it depend on the purpose of names in general, of this particular name, and of this particular utterance of this particular name. The correctness of a name depends on the answer to the question: what do we want from this name, and what character will make it best do what we want it to do? "It is by criteria derived from consideration of the requirements of the referring task that we should assess the adequacy of any system of naming."[3]

The speakers in the *Cratylus* fail to hold firmly in mind that the correctness of a name depends on its purpose. At first it seems that they fully grasp this point, for they call the name a tool and compare it with the shuttle, the knife, and other tools (387-388); and they explicitly ask what we do with the name and answer that we use it to teach each other and to distinguish how things are (388B). Yet the notion of a purpose or τέλος was invented by Aristotle, not by Plato. And Plato in the *Cratylus* does not use his notion of mark or σκοπός, nor his notion of function or ἔργον. (The word "ἔργον" in the *Cratylus* means a product, never a function.) And as his 'Socrates' develops the notion that the name is a tool, it turns into something oddly remote from our conception of the purpose of a tool. Instead of the purpose of the tool, he talks of the Form or εἶδος of the tool; and this Form is something which

[3] P. F. Strawson, "On Referring", *Mind*, n.s., LIX (1950), 341.

we are supposed to apprehend telepathically, without any means of doing so. We are explicitly forbidden to learn from experience what form a shuttle should take: "If his shuttle breaks as he is making it, will he in making another look to the broken one, or to that Form to which he also looked in making the broken one? To the Form, I think" (389A). The inquiry what we do with a name (388B) turns into the contemplation of That Itself which a Name is (389D). The practical study of the business of talking turns into the mystic vision of an object which there are no directions for finding. It is like trying to find out why a hammer should be heavy in the head without considering the use of a hammer. While correctly using the analogy of cutting to show that there is a right and wrong in names (387A), 'Socrates' fails to remark that the rightness of a knife depends on the purpose you have in mind. While correctly saying that the carpenter tries to make an ideal shuttle, he implies that what constitutes an ideal shuttle is nothing to do with its purpose or its success in achieving that purpose. On the contrary, he says that he has shown that our intention has nothing to do with it: "Not such as he may wish, but such as nature demands" (389C). He does not see that until someone wishes for something there is no end, and therefore no means to an end, and therefore no tool either good or bad. Our wish is in the very word "shuttle". Nothing is a shuttle unless someone wishes to weave cloth. The "nature" to which 'Socrates' feels we must submit is in the means, not in the end. Given that we wish for a certain end, then it is no longer a matter of our wishes what is the best means to achieve that end. Thus the correctness and incorrectness of names, far from being independent of us and inconsistent with names' depending on us, depends on our having purposes which we try to fulfill by means of names. A name is correct and a use of a name is correct, roughly speaking, if with it we can achieve the purposes of language, whatever they are, and without it we cannot. It is correct to use the name "*ail*" for garlic in France, if by using this name you can easily buy garlic in France whereas without this name it is difficult.

It is a strange fact that people rarely appeal to the purposes of language when defending the correctness of a piece of language or defending the doctrine that there is some correctness in lan-

guage. The reason why Humpty Dumpty cannot do what he says he can do, namely make words mean what he likes, is that that would prevent him from communicating with other people, which is the main purpose of language. But it is not only children like Alice who fail to see that this is the reason and are confined to dogmatic disagreement with him. Nor is it only ancients like Plato's 'Socrates' who appeal instead to an undetectable Form of the word. What language must be in order to be any *use* is very rarely part of people's early reflections on language.

What is the purpose of a name? The persons of Plato's *Cratylus* fail to see that this question is the key to the correctness of names, and they fail to put the question explicitly (except for the abortive 388B already discussed). Nevertheless, they imply an answer to it. Their answer is false, and that is the great cause of their failure to discover the real correctness of names.

The purpose of a name is to refer us to a thing. The purpose of making a name is to have a means of referring people to something, and the purpose of using the name is to refer someone to it. If you say "Westminster Abbey", you refer your hearer to the Abbey, and you can then go on to describe it to him. If he asks, "What about it?" you can say, "It is falling down". The statement "Westminster Abbey is falling down" describes something; and part of the way in which it does so is that it includes a name which refers to that thing. The description of a thing referred to is done by the statement as a whole; but the reference is done by one part of it, the name, and can be done by the name uttered alone. Both proper and general names refer. Proper names refer definitely to some one particular as such. General names refer indefinitely to one or some or any or all of a set of things.

It is not the job of a name to describe. The job of a name is to refer; and reference is not description, and does not usually describe (though it may describe, as in "that tall man over there"). The utterance of a thing's name does not tell the hearer what the thing is. It only refers him to the thing. It is true that names often suggest descriptions. The name "Gerald" suggests that the nominate is a man. But the suggestion is inessential so long as the word remains a name. A person may call his bad knee "Gerald". A

woman may be christened "Gerald". Humpty Dumpty was wrong to say that a name must mean something.

Since a name does not describe its nominate, there cannot be a "contradiction between the conventional names and the true essences of things". All that can happen is that the description of the thing accidentally suggested by its name may be false. Names do not state the essences of their nominates either falsely or truly; they merely name their nominates, that is, refer to them.

There is a strong feeling that names *ought* to describe as well as to designate. To christen a new thing with an entirely new sound which does not suggest any description is commonly felt to be improper, probably because it brings the arbitrary side of language into prominence. Yet it is convenient to separate the referring function from the describing function, because the description may turn out to be wrong. For example, after we have named a certain species of umbellifer "*gigans*", a bigger species may turn up.

Instead of the feeling that names *ought* to describe as well as to designate, we find sometimes the assumption that names do describe and only describe. We find that the proper function of a name is hardly recognized at all, and instead the name is thought of as if it were a little statement and had the same descriptive function as a statement. This view perhaps lies at the back of the doctrine that all language is metaphorical. It is certainly an essential part of the nature-theory of names in the *Cratylus*.

Early in the *Cratylus*, 'Socrates' approximates names to statements by asserting that if statements are true or false, names are true or false also (385C). In fact, statements are true or false because they describe and assert; and names are neither true nor false because they do not assert or describe, but name or refer.

In the only place where the 'Socrates' of the *Cratylus* explicitly asks what we do with a name, the answer he gives himself is false, because it would be a true account of what we do with a statement, and the function of a statement is not the same as the function of a name. To say that "a name is a tool whose use is to inform each other and to distinguish how things are" (388B) is wrong in the same way as it would be wrong to say that "a carburetor is a tool whose use is to get from place to place". The use of the

carriage is to get from place to place, and the carburetor is part of the carriage; but the use of the carburetor is not the use of the carriage. Similarly, the use of the statement is to inform each other and distinguish how things are, and the name is part of the statement; but the use of the name is not the use of the statement.

All of the great quantity of etymologizing in the dialogue consists in trying to show of each name in turn that it gives a true description of its nominate and was chosen as its name for that reason. Compare, for example, "the essence of the thing revealed in the name" (393D); "what the name means" (397E); "what opinion they were holding when they imposed names on things" (401A). The speakers assume that every *ὄνομα* is an *ἐπωνυμία*, every name is a description. They speak of the *βούλησις* or meaning of the name (421B), which is something different from its nominate. They sum up their etymologizing by saying: "In the names that we have gone through, the correctness was intended to be such as to show what sort of thing each of the realities is" (422D). They ask how the primary names will make things as plain to us as possible, and they seem to imply that names must do this in order to be really names (*εἴπερ μέλλει ὀνόματα εἶναι*, 422E). "The rightness of a name is that which will show what the thing is. . . . Names are uttered for the sake of information" (428E). The rightness of a name is "to be a revelation of a thing in syllables and letters" (433B). "The name is a revelation of the thing" (433D). "The force of a name is to teach" (435D).

When we realize that reference is distinct from description and that the business of a name is not to describe but to refer, the point of looking for enlightenment in etymologies vanishes. Any description suggested by the name is irrelevant, even if the name was deliberately invented by some "onomast" with that description in mind. Whether the man who invented the word "gas" had chaos in mind as a true description of gas is pleasant historical gossip, but nothing to do with the business of the word.

From the account of the name as a description or revelation or *δήλωμα*, the speakers are led to conceive it as an imitation or *μίμημα* (423B, 430A, E), or *εἰκών* (439A); and so 'Socrates'' good remarks on the nature of an image (423-432) come to be at the same time an elaborately false account of the nature of a name.

To imitate is not to name or indicate or mention. A child imitates the behavior of the people around him, and in so doing he is not mentioning their behavior. Imitation is occasionally used as a way of referring; but it is an inconvenient way of referring, used only when nothing better is available. Names are not imitations, but a distinct and much better means of referring to things. In referring to a thing by its name we are making use of a custom instead of an imitation. We imitate things with our bodies only when we are deprived of a customary nonimitative means of referring to them. The tourist in Denmark imitates a man drinking beer only when he cannot think of the Danish name for beer.

Though 'Socrates' makes damaging remarks about the nature-theory in the last part of the dialogue, he never clearly points out this most fundamental falsehood of the theory: that it assumes that the business of a name is to describe its nominate, whereas it is merely to refer to that nominate. (I think it is impossible to say this unambiguously in Platonic Greek.) He never says that we cannot learn about things from their names because things are not described by their names. He says only that it is risky and unsatisfactory to try to learn about things from their names because they may be incorrectly described by their names (435-437, esp. 436B). Even while making the point that many names are in fact unlike their nominates and work by custom or convention or agreement, he still thinks of the name as a "showing" of the thing (δήλωμα . . . δήλωμα . . . δηλοῖ . . . δήλωσιν, 435AB).

Did Plato ever realize the distinction between reference and description and see that the function of names is to refer? Not in any decisive or unmistakable fashion. There is, however, a tendency toward this realization both in the *Theaetetus* and in the *Sophist*. "Each [of the prime elements] itself by itself would be nameable only, not otherwise describable either as existing or as not existing." (*Ὀνομάσαι μόνον εἴη, προσειπεῖν δὲ οὐδὲν ἄλλο δυνατόν*, *Theaetetus* 201E.) The distinction is suggested here and also on the next page with its distinction between *ὄνομα* and *λόγος*. But the point is not developed or firmly grasped; it only floats up for a moment as part of a discussion of the relative knowability of simples and compounds. (The passage assumes the falsehood

that simples are indescribable; but that is irrelevant to the question to what extent it grasps the distinction between description and reference.)

The distinction appears again in the *Sophist*, and more clearly and accurately. "When someone says 'man understands', that you say is a statement, the smallest and first?—I do.—Because, presumably, he is now revealing [δηλοῖ] something about things that are or were or will be. He is not merely naming. He is completing something, by combining expressions with names. Hence we say he is stating, not merely naming; and to this combination we have assigned the name 'statement'.—Right" (*Sophist* 262D). It does not follow, however, that had Plato then gone back to the discussion of names as such, he would have avoided the *Cratylus*' error of regarding them as little statements. It often happens that we imply the right view of a thing when we mention it incidentally, and yet give a wrong account when we discuss it for its own sake.

There is one further indistinction to mention as contributing to the defectiveness of the *Cratylus*' account of names: it fails to realize the difference between establishing a name and using a name. It is one thing to baptize a baby "John" and another thing to refer to him as John thereafter. It is one thing to lay down a meaning for the word "deuterium" and another thing to use the word thereafter. The two things both admit of correctness and incorrectness, since they are both human actions; but their criteria of correctness are different. The failure of Plato's persons to find the real correctness of names is partly due to their failure to hold this difference in mind. The largest part of the dialogue, with its mass of etymologies, is an attempt to justify the supposed original *establishment* of the names discussed; but in 429-431 'Socrates' argues that there can be such a thing as an incorrect *use* of an established name, and the speakers are not aware that they have changed the subject.

We cannot see the real correctness of names unless we consider separately the correctness of their establishment and of their use. The correct use of names, to take that first, consists almost entirely in using the customary name in the customary way. That is because the function of names is to refer, and if we do not use

the customary name in the customary way, we commonly fail to refer our hearers to that to which we want to refer them. Where two customary names are available, the choice between them on any particular occasion may be indifferent, or one of them may be incorrect and the other correct, as for example when one is obscene and the other not. (Obscenity may also effect that the correct thing is not to use a name at all, but to refer by a round-about phrase or a euphemism.) Since customs vary from group to group, the correct name varies from group to group, from place to place, from time to time. Sometimes it is obvious what the correct name to use is; occasionally it is very dubious and there is no satisfactory answer. The wise man respects all customs and follows the custom of those to whom he is speaking. (Among the exceptions to this rule is the case where his hearers, for some special reason, expect him as a matter of propriety to follow a different custom which they understand but do not themselves use.)

The yoke of custom is very heavy in the matter of using names. Most departures from it are summarily punished by unintelligibility, failure to communicate. Those who, discontented with the arbitrariness, or what they sometimes call the "illogicality" of an actual name, try to use something more "rational", become ridiculous or worse. The truly correct, the truly "logical" name is after all the customary name.

This custom, or *νόμος*, which strictly limits our successful speaking, is not the work of any lawgiver, or *νομοθέτης*. We cannot thank or reproach any original namemaker for it. It is part of the great web of culture in which we are held, which we did not make or choose, which we can do little to alter, which we must teach to our children, much of which we can and should learn to love, but much of which, alas, is quite unlovable. So much on correctness in the use of names.

Sometimes new names are successfully made. That is, a new custom is, by deliberate human contrivance, added to the multitude of reigning customs constituting a living language. The word "kodak", which is now a customary part of the English language, was deliberately introduced by G. Eastman (according to the S.O.E.D.); and I have been told that he aimed at inventing

a sound which could be used in different languages. The joke, that an American college president announced that "from today it will be a tradition that . . . ", is popular in England because the English underestimate, and the Americans overestimate, the extent to which human enterprise can deliberately alter human custom and culture.

As to correctness in the invention of names, it is a far more fluid affair than correctness in the use of them; and a far wider area is left open to arbitrary choice. Once we have escaped from the idea that correctness in the invention of names could be the correct description of their nominates, we see that there is no overwhelming, all-disposing principle of correctness in this field, and that to a large extent we are left without any guidance and are compelled to make arbitrary choices. The criteria that determine a good name leave far more things indifferent than the criteria that determine a good knife; and the arbitrary element in language appears nearly naked here.

That there must be much arbitrariness in the invention of new names is concealed through most of the *Cratylus* by the assumption that the business of a name is to describe its nominate. But the doctrine appears, in a distorted version, in 'Socrates'' peculiar view that the form of a name can be the same in different syllables: "If not every lawgiver embodies the name in the same syllables, we must not forget this: not every smith embodies the tool in the same iron, though he makes it for the same purpose" (389DE, retaining the difficult ἀγνοεῖν; cf. 393-394). There is no single form which the sound must have in order to be a name of the thing.

Yet there are rules or principles for good name-making as well as for good name-using. The fact that the job of a name is not to describe by no means involves that it can be made anyhow, or that any name is as good as any other name. A name has a definite use, and anything that has a definite use can be made better or worse for its use. Names need to be short and easily pronounceable, yet distinct from other words. They need to fit well into the phonic and grammatical systems of the language and into the spirit of the language which its customary users vaguely feel. Several principles of name-making are suggested in

my *Definition*, pages 80-92, under the head of "rules for stipulative definition"; for stipulative definition is the establishment or attempted establishment of a new name. I will not repeat them here, but merely add, in conclusion, the two following remarks.

(1) We should probably acknowledge the arbitrary element in language more than we do; for that would encourage us to invent new sounds for our new names, instead of taking an old sound and thus creating a new ambiguity.

(2) I have suggested in my *Definition*[4] that "perhaps we ought not to stipulate unless we are in some sense authorities in the field to discuss which we stipulate". This may recall the statement in the *Cratylus* that "not everyone is a maker of names" (390E). But against the *Cratylus* I wish to say that the proper maker of names is neither the dialectician (unless it is a name for use in dialectic) nor "he who looks to the name in nature of each thing" (390E). The proper maker of names is in each field the influential and well-placed authority in that field; for only he has both the knowledge of what things need to be named and the authority to impose his names.

RICHARD ROBINSON

Oriel College, Oxford

[4] Oxford, 1950, p. 92.

SENSE AND REFERENCE: A REPLY

I

THE intended scope of Frege's distinction between the sense and the reference of names (a technical term that includes predicates, sentences, and other expressions not ordinarily called names) is not easy to determine, despite the unusual clarity of his work. Did he mean that it should be extended to every kind of name, including predicates, or restricted to proper names alone? Certainly he speaks both of a predicate's sense and its reference, but whether this implies a distinction and, in particular, whether these terms are unambiguous, having the same meaning for both proper names and predicates, are questions to which conflicting answers are given.

Mr. Michael Dummett, in his discussion of my article "Frege's Theory of Functions and Objects" (*Philosophical Review*, LXII [1953], 374-390), answers these questions in the orthodox way.[1] He believes that Frege did mean to extend his distinction between sense and reference to each kind of name. Frege, he says, certainly did use "reference" the same way in each context and certainly did mean to ascribe a sense (i.e., in his technical use of the word) to predicates (p. 98).

As Dummett himself remarks, however, it is not easy to cite passages in support of this answer. For although Frege is careful to distinguish the sense and reference of proper names, he is reluctant to ascribe a sense to predicates, and when he does, he usually identifies it with the reference.

Dummett tries to explain Frege's reluctance to ascribe a sense to predicates by supposing he justifiably took the sense for granted and concentrated on explaining the nature of the reference (p. 98). But would Frege also take it for granted that a predicate sometimes *expresses* its sense and sometimes *refers* to it? His notion of a name's sense is, after all, a technical one. Nobody would ever

[1] M. Dummett, "Frege on Functions: A Reply," *Philosophical Review*, LXIV (1955), 96-107.

deny that a normal man has ideas in his mind, colloquially speaking, but whether he has ideas as John Locke, for example, used the word is undoubtedly controversial and ought not be taken for granted. Similarly, nobody would deny that predicates have a meaning, but that does not obviously imply, and may not imply, that they also have a sense as that word is explained in Frege's "Ueber Sinn und Bedeutung."

Besides, Frege could, in a way, take it for granted that sentences have a sense, but that did not keep him from explaining what the sense is, nor how it differs from the reference. No doubt he would have done the same for predicates had he seen how that were possible. But given his analysis of predicates, he could not apply the sense-reference distinction to them without essentially modifying it. To do so he would have had to abandon his most basic view, the "incompleteness" of predicates and concepts—together with all that it implies: in particular, that a concept is the reference of a predicate.

Usually Frege does not distinguish between a predicate's sense and its reference but identifies both with the concept (*Begriff*). This is not linguistic carelessness; rather, the logic of predicates forces him to do this. The notion of a predicate's reference has to have important features in common with that of a proper name's sense. The explanation of why this is true has important bearing upon two puzzling questions—namely, abstract existence and the logical paradoxes.

II

There is only one place, so far as I know, paragraph 32, Volume I of the *Grundgesetze*, where Frege ascribes to predicates a sense distinct from the reference; and there he distinguishes the sense and the reference of names in general. In the other places (e.g., in "Ueber Begriff und Gegenstand") where he speaks both of a predicate's sense and its reference, the two are not distinct, and the words, in fact, mean the same thing.

Moreover, in the passage I mentioned, where Frege, contrary to his usual practice, does distinguish the sense and the reference of each kind of name, he writes mostly about sentences—saying

the sense is a thought and the reference a truth value—and does not explain how we should apply the distinction to other names (a sentence being for him the name of a truth value). He explains this for proper names, in "Ueber Sinn und Bedeutung," and in many places says that a predicate's reference is a concept, but in no place does he ever clearly say how we should think of a predicate's sense (i.e., insofar as it does not mean the reference). Is a concept also a predicate's sense, or is something else the sense? Is a predicate's sense always different from the reference, or do they sometimes coincide? Can a predicate have a sense but no reference? Does a predicate, like a proper name, have an indirect sense as well as a customary one?

In the passage I mentioned, Frege says that the sense of a constituent name within a sentence is its contribution to the sense of the sentence, the thought, and that it is part of the thought; but this does not contain an answer to the above questions. Yet he does not discuss the sense of predicates in any other place, not even in "Ueber Sinn und Bedeutung," for the discussion there of sense and reference, although by far the most detailed that he ever gives, is carefully restricted to proper names—i.e., names of objects. For a discussion of concepts and relations, which are functions rather than objects,[2] and of their names, too, apparently, he refers us to a forthcoming work, doubtless "Ueber Begriff und Gegenstand."

He explains in the former article that a proper name, for brevity called simply a name, has a sense but not necessarily a reference; that a name regularly has both a customary and an indirect sense and reference; that the sense of a name is understood by anyone sufficiently familiar with the language to which it belongs, but "in grasping a sense, one is not certainly assured of a reference" (p. 58); and that the sense "serves to illuminate only a single aspect of the reference, supposing it to exist. Comprehensive knowledge of the reference would require us to be able to say immediately whether every given sense belongs to it. To such knowledge we never attain" (p. 58). (We can evidently have

[2] They are functions "whose value is always a truth value" (p. 155). The reference, here and elsewhere, is to *Translations from the Philosophical Writings of Gottlob Frege*, by Max Black and Peter Geach (Oxford, 1952).

a complete understanding of the sense but never a complete knowledge of the reference.)

Some consequences, more or less immediate, though not actually drawn by Frege, are the following: (a) The sense of a name is always distinct from the reference. (b) The sense determines what object the reference is, so that one who has grasped the sense can decide whether any given object is the reference, provided he has sufficient knowledge of it. (The sense of, e.g., "the teacher of Alexander the Great" seems to be a property belonging to Aristotle alone, who is thus the name's reference. That the sense is a property [or concept] conflicts, however, with Frege's view that properties must be "incomplete," and so he never says it is.) (c) By a definition we can explain the sense and even endow the name with a new sense; but a well-defined and well-understood name—such as "the man over forty feet high," for example—may nevertheless lack a reference.

Of these various features which belong to Frege's distinction between the sense and the reference of proper names, few if any can be extended without conflict to the case of predicates, as he understands them. For example, anyone who understands a predicate is assured of its reference, as I shall show later on. Hence it is not altogether surprising that in the article to which he refers us, "Ueber Begriff und Gegenstand," where concepts and their names, i.e., predicates, are discussed at length, Frege barely mentions the sense-reference distinction, and then only in relation to proper names. Moreover, he uses the words interchangeably, as if believing his technical distinction would not fit the new subject matter; so that his use of terms here is, for example, like that of a naturalist who makes a technical distinction between two kinds of birds and then later, seeing a bird similar to both, calls it by the name of either.

Frege's main purpose in the article, to explain what he understands by a concept, requires that he tell us how to recognize one, and he does so in two ways, appealing each time to grammatical predicates. He says at first that a concept is the reference (*Bedeutung*) of a grammatical predicate (p. 43). It therefore has a predicative nature—this being just a special case of the "incompleteness" or "unsaturatedness" that is the essential feature of all

functions—and hence must be sharply distinguished from the reference of any expression that can serve as a grammatical subject. The reference of such an expression as "the concept *horse*" is, therefore, not really a concept, in spite of appearances; since the expression may serve as a subject, its reference is "complete," hence is an object. The reference of a predicate, on the other hand, being "incomplete," is a function, and in particular, a concept.

Later on, however, he describes concepts in a different way, saying predicates have an "unsaturated" or "incomplete" sense and identifying this with the concept:

> Not all the parts of a thought can be complete; at least one must be unsaturated or predicative; otherwise they would not hold together. For example, the sense of the phrase "the number 2" does not hold together with that of the expression "the concept *prime number*" without a link. We apply such a link in the sentence "The number 2 falls under the concept *prime number*"; it is contained in the words "falls under," which need to be completed in two ways—by a subject and an accusative; and only because their sense is thus unsaturated are they capable of serving as a link [p. 54].

A concept, therefore, may be called in Frege's system either the sense or the reference of a predicate; for he calls it both. Relations are described analogously. The phrase "falls under," he says, has an unsaturated sense and "such words or phrases stand for (*bedeuten*) a relation" (p. 54).

Such an account of concepts (and relations) is not peculiar to "Ueber Begriff und Gegenstand," for in the *Grundgesetze* and also in the late writings,[3] Frege sometimes thinks of a concept as the sense of a predicate (though he would rather think of it as the reference). He says in paragraph 56, Volume II, of the *Grundgesetze:*

> Any object Δ that you choose to take either falls under the concept ϕ or does not fall under it; *tertium non datur*. E.g., would the sentence "Any square root of 9 is odd" have a comprehensible sense at all if *square root of 9* were not a concept with sharp boundaries? Has the question "Are we still Christians?" really got a sense if it is indeterminate whom the

[3] In "Gedankengefüge" (*Beiträge zur Philosophie des deutschen Idealismus* [1923]), and others, according to Dummett, *op. cit.*, p. 105.

predicate "Christian" can truly be asserted of and who must be refused it?

The sentence "We are still Christians," he says in substance, using a rhetorical question, has no sense if *Christians* is not a well-defined concept. From this it follows, as a matter of course, that the concept, as he thinks of it here, is the predicate's sense. If it were merely the reference, its being defective could have no effect upon the sense of the sentence. As he says in "Ueber Sinn und Bedeutung," "If it were a question only of the sense of the sentence, the thought, it would be unnecessary to bother with the reference of a part of the sentence; only the sense, not the reference of the part is relevant to the sense of the whole sentence" (p. 63).

Frege, then, often does think of a concept as both the sense and the reference of a predicate, in spite of what he says in the *Grundgesetze*, Volume I, paragraph 32, about the two being distinct. Dummett thinks (p. 105) this is an aberration of Frege's late writings, whereas actually it occurs in the earlier works as well, as we have seen.

Someone who—like Dummett—thinks Frege intended without qualification to apply the sense-reference distinction to predicates might wish to say that sense and reference are sometimes identical but never synonymous. Just as one and the same person can be both father and employer of some man, so one and the same concept can be both sense and reference of some expression. For proper names sense and reference are always different, for predicates not, but the terms themselves are used the same way for each.

Whether Frege would have accepted this explanation is doubtful. In "Ueber Sinn und Bedeutung" the sense and the reference of a name are held to be distinct, and if he intended an exception for predicates, no doubt he would have said so in some place or other. The truth is, he probably knew that if "sense" and "reference" were to be used in the same way for both proper names and predicates, then they could not possibly coincide for the latter. Hence the fact that he usually lets them coincide for predicates shows he probably did not intend, on the whole, to extend the distinction to predicates.

(i) The sense of a proper name, as we have already seen,

determines what the reference is, if there is one. The sense of, for example, "the teacher of Alexander the Great" belongs to Aristotle, who is thus the name's reference, determined by the sense. Now if the sense of a predicate, for example "man," were a concept (or property), the reference it determined would be what fell under the concept (or that to which the property belonged). But then the sense and reference of the predicate could never coincide, because a concept, on Frege's type theory, can never fall under itself. On his analysis, the sentence "The concept *man* is a man," besides expressing what is false, is not even about a concept. Hence the reference determined by a concept, supposing it were a predicate's sense, could never be that concept itself. To say that sense and reference coincide for predicates would require us, therefore, to abandon the stipulation that the sense determines what the reference is (i.e., if the reference is a concept, as Frege holds).

(ii) Frege says in "Ueber Sinn und Bedeutung" that if we replace one word of a sentence by another having the same reference, this can have no bearing upon the reference of the sentence; and that in indirect discourse, words do not have their customary reference but refer to what is customarily their sense. If this is so—and certainly it partially describes what is meant by the sense and reference of proper names—then again the sense and reference of predicates cannot coincide.

This can be shown in the following way. The sentence

The square root of 9 is odd (1)

refers, on Frege's analysis, to a truth value and, moreover, continues to do so as the grammatical subject varies, so long as the latter has a reference. Consequently the sentence

The sense of "the square root of 9" is odd (2)

also refers to a truth value—evidently to the False, as he calls it. On the other hand, according to his analysis of indirect discourse, the sentence (1), when occurring as part of the sentence

A said the square root of 9 is odd

refers to a thought, not a truth value. This could not be the case, however, if the concept *odd* were the predicate's sense. For according to the doctrine of indirect discourse, the parts of (1) would then refer, as do the parts of (2), to the sense of "the square

root of 9" and to the concept odd. The reference of the whole being a function of the reference of each part and each part of (1) having the same reference as the corresponding part of (2), the sentence (1) as a whole would in this context have the same reference as (2), namely, the False.

By the same argument, the sentence (1) also refers to a truth value when occurring as part of the expression

The thought that the square root of 9 is odd,

supposing again that the sense of the predicate is a concept. We might say, using Frege's language, that the function "the thought that *x*" would then have a truth value as its argument; namely, the value of the concept "*x* is odd" for the argument, the sense of "the square root of 9." Since a thought is not a truth value, the thought that *the square root of 9 is odd* would then be as paradoxical as the concept *horse* that is no concept, discussed in "Ueber Begriff und Gegenstand."

Actually, the argument above does not depend upon the assumption that sense and reference coincide for predicates but upon the assumption that a concept is the customary sense of a predicate.[4] No matter what the customary reference might be, if the sense were a concept and were *referred to* in indirect discourse by the predicate, the relation of the concept to the logical subject would, according to Frege's principles of substitution, be that of a function to its argument. Then, since a concept is a function "whose value is always a truth value," the indirect reference of a sentence would be a truth value instead of a thought. Whether this would also be true if a predicate did not refer in indirect discourse to its customary sense would depend upon what it did refer to, if to anything; but in any case, the notion of a name's sense would have been changed. As for Frege's own view of the indirect reference of a predicate, it is not known; he never says to what a predicate in indirect discourse refers. For him the important thing about a predicate is the associated concept, sometimes called the sense, sometimes the reference, since—as I

[4] We should not infer from this, however, that a concept is therefore the customary reference of a predicate. Frege did not extend his theory of indirect discourse to predicates.

shall show later—it is similar to both, as these are explained for proper names.

Dummett thinks that when Frege says a concept is the sense of a predicate, he does not mean it (p. 105); and probably this would be true had he meant to extend to predicates the sense-reference distinction. Then a predicate's sense—which might perhaps be some other function than a concept—would be relevant for determining the reference, and an understanding of the predicate would not imply a reference. But as it is, a predicate's reference, which is a concept, does not have to be determined by something else, any more than does the sense of a proper name; and, moreover, an understanding of the predicate implies a reference, as the understanding of a proper name implies a sense.

The simplest way to extend to predicates the sense-reference distinction as it is explained in "Ueber Sinn und Bedeutung" is to let the sense be a function whose value, unlike that of a concept, is not a truth value,[5] and to let the reference be the class the sense determines, or if not the class, then the *Wertverlauf* (which itself, however, is a type of class).

To each argument of a function $F(x)$ there is correlated the value of the function for that argument (as in plotting a graph in analytical geometry). Thus

$$\begin{aligned} F(a) &: a \\ F(b) &: b \\ F(c) &: c \\ &\cdot \\ &\cdot \\ &\cdot \\ F(n) &: n \end{aligned}$$

The class of all these pairs of objects is the *Wertverlauf* determined by the function.

In this way of applying the sense-reference distinction to predicates, which is essentially that of Alonzo Church,[6] the reference is determined by the sense, and the nature of the sense is not

[5] Or we might simply abandon the stipulation that a concept be a function whose value is a truth value and say its value is a thought.

[6] Cf. his review of W. V. Quine's "Notes on Existence and Necessity" in the *Journal of Symbolic Logic*, VIII (1943), 45-47.

in conflict with the doctrine of indirect discourse. Frege himself, however, did not make such an application, probably because he thought a "logical whole" had to contain one or more "incomplete" parts, a view he would have had to give up if a predicate referred to a class or a *Wertverlauf*, since these, being objects rather than functions, are not "incomplete." His reasons for thinking a predicate must refer to something "incomplete," insofar as these concern an analysis of language, will be given later. Here, as in almost every other case, his reasons are excellent.

Church's method of applying the sense-reference distinction to predicates is perhaps not the only one possible; but whether it is or not, the doctrine of "incompleteness" requires that predicates have a sense and a reference—if at all—in a way different from proper names.

(i) One of its consequences is that the reference of a predicate, i.e., the concept, cannot be specified, for (P being a predicate) any assertion of the form "P refers to x" must be false. The predicate "horse," for example, does not refer to anything we can name, not even to the concept *horse*, because the latter, Frege says, is really an object in spite of its name. Since this is not true for proper names—e.g., "Aristotle" refers to Aristotle—they and predicates cannot have a reference in the same way.

Let N be a proper name and P a predicate. Then whereas the assertion

N has a reference

implies

$(\exists x)$ (N refers to x),

the assertion

P has a reference

does not correspondingly imply

$(\exists x)$ (P refers to x).

That is, whereas N, if it has a reference, satisfies the condition

$(\exists x)$ (y refers to x),

P, on the contrary, does not, even if it has a reference. Since "P refers to x" is false for any x, existential generalization with respect to x is never valid.

Suppose that although no one whom we could possibly name ever did any work for Mr. B, people who knew this nevertheless said, "B has an employee"—meaning, "For anyone at all, it is determinate whether he would apply to B for a job." Such an employee would be like a predicate's reference, in Frege's sense of the word, for to say a predicate has a reference is to say that for any object, it is determinate whether the predicate applies to it.[7] Just as "B has an employee" does not imply "There is someone whom B employs," so "*P* has a reference" does not imply "There is something to which *P* refers."

(ii) A second consequence of the doctrine of "incompleteness" is that a predicate is always to be defined by defining its contexts, and a concept by specifying what properties something must have in order to fall under it. If we say "*x* is a square" means "*x* is a rectangle in which the adjacent sides are equal," we must think of the "*x*" as indicating some unspecified object. The definition explains the predicate "square" for such contexts as "*a* is a square," "*b* is a square," and so on, but not for contexts where its "empty space"[8] is not filled—e.g., "*x is a square* is a concept"; that is, not for contexts where "square" is not used as a predicate. As an alternative definition, we might say: "A square is a rectangle in which the adjacent sides are equal." Frege calls this a definition of the concept *square*,[9] i.e., of the concept to which "square" refers.

From this it is clear that a predicate has a reference if it is adequately defined. Consequently, the only kind of condition that Frege could lay down for predicates' having a reference is the kind he lays down in paragraph 29, Volume I, of the *Grundgesetze*; namely, that a predicate has a reference if the sentence formed of it and a proper name always has a reference (i.e., a truth value) whenever the name has. For predicates of ordinary speech, which are not defined as precisely nor for so wide a range of objects as the predicates of the *Begriffsschrift* he had in mind, this condition is too strict and would have to be relaxed somewhat. Whether certain objects are, for example, red or not, is probably indeterminate, but it is nevertheless obvious that Frege would not

[7] Cf. *Grundegesetze*, vol. I, par. 29, where this is implied.

[8] *Ibid.*, par. 21. [9] *Ibid.*, p. xiv.

care to say that such predicates as "red" are without a reference, for he often speaks of their reference.

Thus a predicate's reference is very much like a proper name's sense. Whereas a definition endows a proper name with a sense (though not with a reference), it endows a predicate with a reference. Frege says that in grasping the sense of a proper name, one is not certainly assured of a reference. This is not true for predicates. Anyone who understands the sense of a predicate knows whether to assert it of various kinds of objects, provided he has the relevant facts about them at hand. A person who could not say whether Indians are red would not have grasped the sense of that predicate—perhaps through lack of familiarity with English; but if he has grasped its sense, he should know it has a reference. Indeed, there is no difference between them.

We can now see why Frege is indifferent about calling a concept both the sense and the reference of a predicate. For if by a definition predicates are endowed with a reference, then the distinction between their sense and reference has been obliterated.

We can also see why Frege's view of concepts is extensional.[10] To describe a given concept, we say that if an object has such and such characteristics, it falls under the concept. This is like describing a man by stating what properties someone has to have in order to be a relative of him. Imagine that someone said: "I cannot describe my coat directly, but I can give you the measurements of everyone whom it would fit (or a list of everyone whom it would fit)." This would give us some information about the coat's size, but not about its other features. Still, if that were all we could say about coats, and if we required a criterion of identity, then we should have to say that coats fitting the same people were the same coat.

In my article discussed by Dummett, I took the position that the peculiarities ascribed by Frege to concepts are really an appearance, caused by looking at their names—or rather at what are taken to be their names—from a certain standpoint. If this position is correct, predicates cannot have a reference in the way proper names have. Dummett is wrong, however, when he says (p. 98) that on my interpretation of Frege, a concept is the sense,

[10] Cf. p. 80.

not the reference, of a predicate. My interpretation is, as I have already explained, that a concept may be called either the sense or the reference of a predicate. Moreover, I think, as I shall explain later, that Frege had a good reason to say that predicates have a reference. Certainly I do not object to Frege's—or to anyone else's—calling a predicate a name, though a peculiar kind of name, so long as this does not obscure important logical differences between predicates and proper names. It sometimes causes us to think, however, that, for example, the predicate "master" and the proper name "Aristotle" are more alike than they actually are. I believe this is one reason that Frege tried to explain the seemingly peculiar "incompleteness" of predicates by attributing a corresponding peculiarity to concepts. But that we need something so peculiar as the "*Ungesättigtheit der Begriffe*" to account for the logical differences between predicates and proper names hardly seems necessary. The doctrine does not explain what it is supposed to, anyway. Essentially, it means that concepts cannot exist apart from their arguments; but that fails to explain why concepts cannot be named apart from their arguments. To name something is not to remove it from its setting.

Mr. Peter Geach, in his article "Class and Concept,"[11] says he holds with Frege that the distinction between concepts and objects is founded in the nature of things, and he argues that if we try to blur it by treating a concept as an object, we involve ourselves in a paradox. His argument is substantially the following:

Grant that every predicate refers to a concept; and suppose that a concept is an object and can accordingly be given a proper name. If "*D*" is the proper name of some concept, the expression "*D*(*x*)" means "*x* falls under *D*." Now since every predicate refers to a concept, so, in particular, does "*x* falls under *x*" and hence "not-(*x* falls under *x*)." Call the concept to which the latter refers by the proper name "*W*." Then "*x* falls under *W*," or "*W*(*x*)," will mean "not-(*x* falls under *x*)." Consequently, "*W* falls under *W*" will mean "not-(*W* falls under *W*)," which is an absurdity, Geach says.

Suppose that a shopkeeper sells two kinds of jewelry—namely,

[11] *Philosophical Review*, LXIV (1955), 561-570.

A's and *B*'s. The *B*'s are marked with a price and are sold either by themselves or attached to an *A*. The *A*'s are not marked with a price and are not sold alone. To find the price of an *AB*, the merchant uses the rule: multiply the price of the *B* by two. But what if he attaches an *A* to itself and tries to compute its price by the old rule? He might reason: An *AA* is simply an *A*; so by my rule, the price of an *AA* is always twice what it actually is, which is absurd. What should the merchant say?—"I should not have done what I did. I have gone against the nature of things. The true state of affairs has now asserted itself"?

In this example we see the kind of circularity that Russell takes to be the essence of many paradoxes. To compute the monetary value of certain objects, using the given rule, we should have to know it already. But if we do know it already, the rule is unnecessary, and if we do not know it, the rule is useless. Indeed, the rule could be used only if the notion of monetary value were, so to speak, ambiguous. If using a rule *R* we compute one value for the object, and then using a second rule *R'* we make use of the first value to compute a second, we get conflicting values; but that should not be surprising.

Geach—and others, of course, who derive Russell's paradox for properties—introduces an apparent relational predicate, "falls under" (or some logically similar phrase such as "satisfies" or "belongs to"), and an apparent name of the form "the concept *F*"; and uses them in such a way that "*x* falls under the concept *F*" means simply "*F*(*x*)." The sentences "Socrates is a man" and "Socrates falls under the concept *man*," for example, both make the same assertion about Socrates. We should expect, therefore, to find that "is a man" and "the concept *man*" are logically different, for otherwise "falls under" would have no function. If the former refers to a concept, then the latter, it seems, does not—or at least not to the same concept; and the question arises, to what does it refer?

Geach, unless I have misunderstood him, would say it does not refer to anything, for it is an incomplete symbol; and I think this is correct. If so, the phrase "falls under" does not refer to a relation after all. We cannot separate out "falls under" from predicates such as "falls under the concept *man*" and "falls under

the concept *horse*" and treat it as a predicate in its own right. Nor can we ask whether a concept falls under itself.

To take a particular case, suppose we ask: Is "the concept *man* is a man" (or using Frege's notation, "*is a man* is a man") true or false? The phrase "the concept *man*," which is ordinarily the accusative of its sentence, is here the subject and so cannot be got rid of. What it means is, therefore, not at all clear. One might suppose that we could explain it by explaining the predicate "man," but this is not true. A predicate is explained by explaining what values it expresses for the various possible arguments, but it is not explained for cases where it occurs without an argument—i.e., with its "empty spaces" unfilled.

Nevertheless, what we want is that "the concept *man*" refer to the concept expressed by the predicate of the sentence "The concept *man* is a man"; and it seems as if we could explain the phrase in that way. But the trouble with such an explanation is that since concepts with different extensions are different concepts, we cannot determine (i.e., if we accept the explanation) to which concept the predicate refers until we first determine to which concept "the concept *man*" refers, and this kind of circularity conflicts with the nature of predicates. Does "the concept *man*" refer to the concept containing the reference of that expression within its extension, or not? To answer the question, we should have to know the reference of "the concept *man*," and we have not yet said what it is. The reference varies, depending upon which answer we give to the question whether the concept *man* falls under itself.

If someone asks, "Is cow *A* black or not?", we should like to know which cow *A* he has in mind before we try to answer. If he said, "Answer the question first, and then you shall know," his question would be similar to the one above. But this shows that "the concept *man*" cannot refer to a concept—i.e., that it cannot function as predicates do. Jesus Christ may or may not have been a man, but how we answer the question has no bearing upon the sense (or reference) of the predicate "man." In general, the definition of a predicate is logically prior to the question whether it applies to any given thing; and consequently its sense is not affected by how the question is to be answered. As Russell says,

"A function is not a well-defined function unless all its values are already well-defined."[12] (A predicate is a special case of a function, as Russell uses the word.)

To say that a predicate refers to a concept is merely to say that the predicate's values for a certain class of arguments is determinate, and this presupposes that the predicate be used as a predicate. (Its essential characteristic, Russell says, is ambiguity.)[13] If we use the predicate as a subject, it cannot in that context refer to a concept, and what it does refer to is not known, because we have not given it a meaning. On the other hand, in expressions such as "*x* falls under the concept *man*," the term "man," although not used as a predicate, is nevertheless clear, the reason being that the whole context—which means "*x* is a man"—is clear.

The expression "falls under" is, therefore, merely a linguistic device for reading an expression of the form "$F(x)$," and whatever the latter means will be what "*x* falls under *F*" will mean. If the latter means "not-(*x* falls under *F*)," that is what "*x* falls under *F*" will mean. If it means nothing, that is what "*x* falls under *F*" will mean.

Until we specify what predicate "*F*" represents, the expression "$F(x)$" does not refer to anything; hence, neither does "*x* falls under *F*." To say that "*x* falls under *F*" is a predicate is, therefore, like saying "*x* is a man" is a proposition. It is not itself a proposition, but it becomes a proposition when the "*x*" is replaced by a name. Similarly, "*x* falls under *F*" is not itself a predicate, any more than is "$F(x)$," but it becomes one when "*F*" is replaced by a predicate. If we wished to call "*x* falls under *F*" a predicate, we should have to call it a variable predicate, as $G(x)$—though not log *x* or sin *x*—might be called a variable function. In "*x* hits himself," we can think of the predicate as "( ) hits ( )." If we do the same with "*x* falls under itself," we get "( ) falls under ( )." But "*x* falls under itself" means "$x(x)$," and should "( )" be called a predicate? How would one express it aloud? By saying nothing?

To say that "*x* falls under itself" is a senseless expression does not, of course, explain the paradox (though it will make its

[12] *Principia Mathematica*, 2d ed. (Cambridge, 1950), p. 39.
[13] *Loc. cit.*

derivation impossible), for many expressions are senseless but do not generate paradoxes. But then we do not try to explain most expressions by their own negations, either. Someone who introduced the expression "$G(x)$" might define it by giving a rule determining the value it expressed for each argument—e.g., to get the value, take the square of the argument. Then $G(x)$ would be the function $y = x^2$. For the expression "$W(x)$," used above in deriving Russell's paradox, the rule is: Take the negation of what results from having the argument x fall under itself. This is obviously circular, since "x falls under W," meaning "$W(x)$," already contains the words "falls under." According to the rule, the value expressed by "$W(W)$" is the same as that expressed by "not-$W(W)$"; so that to find the former, we should have to know it already, and then "$W(x)$" would have a meaning different from the one we tried to give it by stating our rule. Using two different rules, we might get conflicting values for "$W(W)$"; but then $W(W)$ if and only if not-$W(W)$ would be an ambiguity, not a contradiction.

Since the sentences "Socrates is a man" and "Socrates falls under the concept *man*" both make the same assertion about Socrates, it is not surprising that "is a man" and "the concept *man*" are logically different. The consequences of treating them as if they were logically identical are, perhaps, about what we ought to expect—i.e., senselessness and circularity. So when confronted by certain paradoxes, we can say in a way, echoing Frege, "Die Wahre Sachlage hat sich geltend gemacht."

The function of "falls under" in a sentence is, according to Frege, twofold. It links together the grammatical subject and accusative; and it refers to a concept that links together the logical subject and accusative. It is "incomplete" and its sense is "incomplete." It is a peculiar name of a peculiar thing—like all predicates. Thus Frege's reasons for saying predicates and concepts are "incomplete" are closely related to the logical differences between such expressions as "is a man" and "the concept *man*."

These differences show, however, that there is an incompatibility among the following principles of the name relation, all of which have been, and still are, widely accepted; and this, too, is closely related to the doctrine of "incompleteness."

(i) If one can quantify with respect to an expression, it is a name, i.e., it has a reference. As Quine says, "A word *W designates* if and only if existential generalization with respect to *W* is a valid form of inference."[14]

(ii) Two names with the same reference are interchangeable, *salva veritate.*

(iii) What a name refers to can always be described as the so-and-so.

By the first principle, the predicate of "Socrates is a man" must be a name with a reference; for we can infer "There is an *f* such that *f* (Socrates)," as we can infer "There is an *x* such that *x* is a man." Suppose, then, we ask what the reference is. Frege says, "A concept." To the question "What concept?", the natural answer would be "The concept *man*"; but this, though in accord with the third principle, is in conflict with the second. The result of substitution, "Socrates is the concept *man*," or (taking the predicate in a different way) "Socrates the concept *man*," is, if not senseless, then false. As Frege says, in a similar context, of the expressions "square root of 4" and "the concept *square root of 4*," they "have an essentially different behavior, as regards possible substitutions . . .; i.e., the reference of the two expressions is essentially different" (p. 50).

Indeed, we cannot replace the predicate of the original sentence "Socrates is a man" by even "the reference of 'man' "; Socrates is a man, but he is not the reference of the predicate "man." And when Frege says the reference of a grammatical predicate is a concept, this, too, is false, by his own principles. The grammatical subject "the reference of a grammatical predicate" is a "complete" expression whose reference is thus an object, not a concept. He seems, however, to have overlooked this.

Frege says, in substance, that predicates are names, but peculiar names, not subject to the third principle. One consequence, as we have seen, is that predicates do not have a reference in the way proper names have. Church and Carnap, on the other hand, would rather give up the second principle, so far as predicates go, and keep the first and third.[15] The predicate of

[14] "Designation and Existence," *Journal of Philosophy*, XXXVI (1939), 706.
[15] Cf. R. Carnap, *Meaning and Necessity* (Chicago, 1947), p. 19.

"Chicago is a city," they say, refers to a class, the class of cities —even though "Chicago is the class of cities" does not express a true thought, nor "Chicago the class of cities" any thought.

There is no necessary reason, however, to accept the first principle, with its consequence that a predicate is a name, though a strange one. We are not forced to assimilate quantification over predicates to quantification over proper names. But whatever we do, it will not make predicates any more nor any less like proper names than they already are, and quantification with respect to predicates will continue to be a valid operation.

Frege thought, with the analogy of proper names in mind, that since one can quantify over predicates, they must have a reference, which he called a concept. What is important for his theory of quantification, however, is that the concept be there; and considering how he conceived an expression's sense, he could easily have said that a predicate, though it is not a name and has no reference, can nevertheless be quantified over because it expresses a sense, a concept. An expression's sense, or its reference, as he conceived it, is a thing correlated with a word and has to be, to be either.

According to the condition (cited above) that Frege lays down, however, a predicate has a sense (or reference) if it is well-defined, and this—which accords with common sense—does not imply that the sense of a predicate is just another kind of object, analogous to the reference of a proper name but perhaps less substantial. If a predicate is defined, we can say, if we wish, that it denotes or expresses a concept and that the concept exists. But to know that a concept exists is merely to know that a predicate is used according to rules. Every time we say something such as "*a* is (or is not) a man," we can infer that there exists a concept, or property, and nothing could be more certain, nor less extraordinary, than the existence of properties; for it involves nothing more remarkable than the fact that we can make up and follow rules of one sort or another—play games, as it were. Therefore, whether we speak a language that "countenances" (to use Quine's word) quantification over predicates or whether we do not is of no concern whatever, at least for ontology. That we can quantify with respect to predicates does not imply that

our language contains a regrettable bias in favor of realism.

Frege sometimes compares the concept to Kant's synthetic apperception and says that it gives to things their form and structure:

> The concept has a power of collecting together far superior to the unifying power of synthetic apperception. By means of the latter it would not be possible to join the inhabitants of Germany together into a whole; but we can certainly bring them all under the concept "inhabitant of Germany" and number them [*The Foundations of Arithmetic* (Oxford, 1950), p. 61e].

Now certainly we can understand the predicate "inhabitant of Germany" and can determine whether "*a* inhabits Germany" is true and what the number of such inhabitants is; but if the existence of the concept "inhabitant of Germany" implies anything beyond this, it has not been explained. Even Quine's nominalistc language, not allowing quantification with respect to abstract terms, would require the existence of concepts in the sense above; for it would have to contain the possibility of true and false statements.

WILLIAM MARSHALL

The University of Texas

THE CONCEPT OF EMPIRICAL KNOWLEDGE

IN THIS discussion I shall interpret the expression "empirical knowledge" so as to exclude from its field of application not only pure mathematics and logic but also all those immediate apprehensions of sense qualities and relations which we express by sense-datum statements. These exceptions are, some think, arbitrary. I do not think they are, but I shall not argue the point. It is sufficient to say that it will simplify the subesquent discussion if it can be taken for granted that empirical knowledge is neither a priori on the one hand nor direct apprehension of sensuous content on the other. A case of empirical knowledge is one in which a characteristic is imputed to a subject matter which transcends the immediate experience of the knower. In all such cases a claim is made which can be mistaken and for which a justification can significantly be asked.

We may further elucidate what we mean by classifying a judgment as empirical knowledge by saying, as a first approximation, that such judgment is true on the one hand and justified on the other. I shall not attempt here to discuss in detail what is meant by truth. But there is a point which needs to be made in connection with the truth of empirical knowledge. The word "true" as applied to empirical cognitions very often has the force of "approximately true." Thus, if I say of a physical thing that it is spherical in shape, my description may be sufficiently like the truth to satisfy our practical and intellectual requirements so that we say that the description is true in spite of the irregularity of the shape described.

Now, the expression "approximate truth" is a vague expression. We have no precise criterion which is both sufficient and necessary to determine, in all possible cases, whether or not a proposition is approximately true. But the vagueness of an expression does not, of course, mean that it is without significance. It would appear to follow that the expression "empirical knowledge" is also vague, for that term is applicable to cognitions if and only if they are not only justified but also true or approximately true. Concepts or

symbols are instruments: some instruments we want sharp, others dull. Vagueness is a classification of logic without pejorative significance.

But if vagueness attaches to what we understand by an acceptable approximation to truth, we shall find that vagueness attaches also to the notion of a justification, so that the concept of empirical knowledge is doubly vague.

Justifications are of different kinds. For justifications are arguments, and there are many kinds of argument. Also, since arguments differ in cogency, some justifications may be called stronger than others. When we claim to have empirical knowledge that X is the case, we claim not only that X is the case, but also that we are justified in so thinking. Empirical knowledge, we repeat, is not certifiable a priori; it refers beyond the sensuously presented content of observation; it is at least approximately true and it is justifiable. When we claim to have empirical knowledge that X is the case, we serve notice that we are to some extent ready to present a justification for the acceptance of X which is strong enough to satisfy the requirements of those whom we ought to consider. But presumably we are not absolutely clear whom we ought to consider. One might say that we need to consider impartial and reasonable critics, but we cannot be said to have formulated definite criteria for the application of these expressions. Still, there are situations in which we understand how to apply these terms. They are significant expressions.

Let us now consider more closely the kind of argument which may be employed correctly to support a claim to possess empirical knowledge. Every argument has premises and a conclusion. What is the conclusion of an argument which supports an empirical knowledge claim? Is it perhaps the proposition that we know a certain proposition to be true? Not necessarily and not usually. Generally speaking, the conclusion of such a justification is not that we know a proposition, but rather it is the proposition itself which we claim to know. When I present a strong argument for a proposition, p, I thereby present, also, a justification of my claim to know that p is true. Indeed, every justification of an empirical knowledge claim could be expressed as an argument in which the conclusion is the proposition whose truth we claim to know.

Let us now consider the premises of such a justification. Does it suffice that they be believed to be true? Must they, perhaps, be known to be true? Or is it required that they *be* true in order to provide justification for an empirical knowledge claim?

When a person justifies his claim to know, he surely affirms certain propositions and thence infers with some degree of cogency that some other proposition holds true. The premises being affirmed are evidently believed true. If the argument is impeccable from the standpoint of logic and the premises are believed, has a justification been presented for the claim to know the conclusion? It is understood that the conclusion may be false and the claim to possess knowledge may be mistaken. The question before us is only whether the claim to know has been justified.

Some will say that if the premises of the justification are not known to be true, we have not justified our claim to know. They think that there is a difference between exhibiting the implications of our antecedent beliefs and justifying acceptance of the conclusions drawn from those beliefs. If our beliefs are without justification, they think, then no argument utilizing those beliefs as premises will afford justification for our claim to possess empirical knowledge. And this view is at first glance plausible. That is, it fits in with traditional preconceptions.

But if the premises of our justification must be known in order for our justification to justify, how can this be reconciled with the fact that not everything can be demonstrated by argument, since arguments must have premises? Empirical knowledge is a posteriori, transcendent, justifiable, and true. The transcendence of empirical knowledge means that it is not an implication of sense-datum propositions. The a posteriori status of such cognition means that it is not certifiable by logical analysis alone. Hence, the premises in our justification of empirical knowledge cannot consist exclusively of a priori statements and sense-datum statements. Therefore, if the premises of a justification must be known to be true, then some of our premises would have to express empirical knowledge in order to justify a claim of the sort here being considered. No principle of logic and no datum of experience, nor any combination of these, will enable us to justify a claim to have empirical knowledge of a matter of fact transcending

immediate experience. But it is senseless to say that we must have empirical knowledge of the truth of a premise in every argument which justifies a claim to possess empirical knowledge.

However, recent investigations into the subject of probability may appear to have put this matter in a new light. It is apparent that justifications need not be, and usually are not, completely conclusive. It appears to be true that we can often provide sufficient justification for empirical knowledge claims by appealing to considerations of probability. Is it perhaps possible that all justifications of empirical knowledge claims which take the form of probability arguments have premises which *are* known to be true? For it might be the case, as some think, that probability statements are a priori determinable, so that from the data of sense and analytic knowledge of probabilities we might hope to establish cogent though not absolutely conclusive justification for our empirical knowledge claims.

The view that probability is a priori determinable takes many forms, so that writers as diverse as Laplace, Keynes, Carnap, Jeffries, and C. I. Lewis may, despite their divergences, be called exponents of this view. In the following discussion it will not matter what precise form we give to the definition of probability or whether we give it any definition at all. Nor will we consider whether probability admits of quantitative measurement or whether the familiar mathematics of probability applies to such a priori determinable relations as are here in question. The point to be made is quite general and does not, I think, depend upon such particularities.

Suppose that our data are formulated by the statement D and that H is a proposition which we claim to know is true. On the view we are examining, we would justify our claim to know H by certifying a priori that H is highly probable in respect to D. In order to direct attention upon essentials, I propose to concede, for the sake of argument, that all of our data are incorporated into the statement D, so that no neglect of relevant evidence may properly be imputed to the person who claims to know that H is true. And also we may in the same spirit concede that a meaning of probability has been specified, so that it may be established a priori, be it intuitively or by argument, that H is highly probable

with respect to *D*. Let us, having made these concessions, ask for the justification of the claim to know the truth of *H*. Such a justification, we have seen, is an argument having *H* for its conclusion. But so far such an argument has not been mentioned. What has been conceded is that it can be shown a priori that *H* is highly probable in respect to *D*. But this is not empirical knowledge. We have not yet been given any argument, neither a conclusive nor an inconclusive one, in which *H* has been concluded. All that we have so far is the statement *D*, which is supposed to be a complex sense-datum statement, and also the a priori statement "*H* is probable in respect to *D*." But no conclusion as to the truth of *H* has yet been drawn. But why not draw the conclusion, draw it provisionally if you like, draw it with mental reservations, but draw it? And if we do so, then we shall have a paradigm of what may be termed a relatively cogent justification of the claim to know that *H* is true.

Let us not pass too quickly over this last point. A new rule of inference has evidently been adopted: provisionally to infer *q*, if it is proved that *q* is highly probable with respect to the entire corpus of our evidence, *D*. But this rule is not a rule of logic. Nor is this rule utilized in the formal algorithm of probability. On the other hand, it will not be suggested that such a rule is intended as an arbitrary stipulation. An arbitrary rule of inference obviously would not be more efficient than guessing, so far as forming true judgments is concerned, and in any case, it has nothing to do with the justification of a knowledge claim. If we bear in mind that the objective of inquiry is knowledge and that empirical knowledge is at least approximately true, then we will not deliberately conform our reasoning to such a rule of inference unless we anticipate some sort of correlation between reasonable belief, which we justify by reference to the entire corpus of our data, and approximately true belief. Whoever systematically uses such a rule of inference expects, albeit implicitly, that such a correlation will tend to prevail. Otherwise he would not be surprised, as he is, when highly reasonable beliefs prove false, as sometimes happens. Our acceptance of this rule of inference reflects our expectation that only exceptionally will a proposition which is highly probable with respect to the totality of our

evidence fail to possess at least approximate truth. Such an expectation is vague in many respects, but without it the rules of probability have nothing to do with the justification of knowledge claims.

Such an anticipation of nature may be the product of much past experience, but clearly it would be senseless to speak of justifying such an anticipation of nature. Expectations are not certifiable a priori. They are not sense-datum protocols. And this particular anticipation of nature is constitutive of our sense of evidence and is therefore not an example of empirical knowledge for which a justification may fairly be asked or reasonably expected. And here we may note that the circumstance that this expectation, which is constitutive of our sense of evidence, is not itself a kind of knowledge does not indicate a reason for reservation or doubt, but only the recognition that the work of justification through probabilities has material presuppositions.

We initiated this discussion by asking whether it is possible for the premises of every justification of empirical knowledge claims to be known to be true. We have seen, even if probability is an a priori determinable relation, that material expectations are at least presupposed by such justifications. There is merit, perhaps, in saying that such presuppositions are not premises of these arguments—but at least we may say that the work of justification by probability argument presupposes anticipations of nature which are not in turn justifiable by probability. This consideration is familiar to every student of Hume.

It is not necessary, then, in order for a justification to justify, that both its premises and presuppositions be known to be true. What is necessary is that it not be known that either the premises or presuppositions of the argument are false. Not known by whom? Not known by anyone at all.

Now we ask whether the premises and presuppositions of a justification must *be* true in order for the justification to justify? And this question is closely connected with the question "Must we, when we correctly claim to have empirical knowledge, be in a position to *know* that our claim is justified?" If we hold that in every case of empirical knowledge we know, or are in a position to know, that we are justified in claiming knowledge, then the

paradoxical proposition follows that a knowledge claim may be justified by the help of an argument whose premises or presuppositions are false. We can see that this paradoxical conclusion follows by the following consideration:

If we know that we are justified in claiming knowledge and that a justification must have true premises and true presuppositions, then evidently we can know that the premises and presuppositions of the justification are true. But (this we have already established) we cannot know in the case of every justification that its premises and presuppositions are true. Hence, it follows that if we know that we are justified in claiming knowledge, then our justification need not always have true premises or true presuppositions. Hence we have this result: if we hold that every case of empirical knowledge is one in which the knower knows that he is justified in claiming knowledge, then we must hold that it is not essential to justification that its premises or presuppositions be true. But it is paradoxical to say that if one affirms a true proposition for bad reasons, such affirmation still may be a genuine case of empirical knowledge. Let us consider an example.

If a man believes that his friend is, in general, faithful, he is justified, we are apt to suppose, in believing in the fidelity of his friend on some particular occasion. But now suppose that although his friend has kept faith on this particular occasion, he had, in fact, constantly broken faith in the past. Does our hero know, does he have empirical knowledge of the fact that his friend has behaved faithfully on this occasion, when his justification consists of the falsehood that his friend is faithful in general?

Evidently we need to draw a distinction between a justified knowledge claim and one for which a justification has been given. A knowledge claim for which a justification has been given may still not be justified if the premises or presuppositions of the justification are untrue. Hence, in the example given, the knowledge claim was in error for the reason that it was unjustified. A justification was given, but since its premise was erroneous, the claim to know was not in fact justified.

We saw that if we always know whether we are justified in claiming knowledge, then it is not necessary that the premises

and presuppositions of our justifications be true. And this is the same as saying that if the premises and presuppositions of justified knowledge claims are always true, then we do not always know whether or not we are justified in laying claim to empirical knowledge.

Then how can we *ever* know that we are justified in claiming to have empirical knowledge? We can know that we *are* justified in those cases in which we know that the premises and presuppositions of our justification are true. But even when we do not know that we are justified, we may *be* justified and we may claim to be so justified. Indeed, whenever we present a justification for a knowledge claim, we implicitly *affirm* that our claim is justified, but we cannot know that it is justified unless we know that the premises and presuppositions of the justification are true. Hence, we do not know that we know except when we know that our premises and presuppositions are true.

In general, it does not follow from the fact that we know, that we know that we know. Here, I think, is a respect in which empirical knowledge differs from other types of knowledge. If I know that twice two is four, I know that I know it. If I know that this datum of sense is blue, I know that I know it. But if I know that men are mortal, I may not know that I know it. For knowledge that men are mortal may be justified by an inductive extrapolation. This extrapolation presupposes the expectation that well-confirmed hypotheses are for the most part at least approximately true. This expectation is not empirical knowledge, though it may be a precondition of empirical knowledge.

If we do not know the presuppositions of a justification to be true, we may *have* knowledge but we do not *know* that we have knowledge. We may nevertheless be convinced that we have knowledge even when we do not know that we have knowledge. We may be convinced that we are justified in claiming to have knowledge even when we do not know that we are justified.

Furthermore, we can see a priori that if there is to be any empirical knowledge at all, then some empirical knowledge must be claimed in advance of the knowledge that it *is* knowledge. And we can see a priori that if any justifications of empirical knowledge claims are to be presented at all, then we must claim,

in the case of some justifications, that they *do* justify in advance of the *knowledge* that they justify.

There is a relation between the views expressed in the preceding discussion and what may be called philosophical skepticism. Philosophical skepticism is an expression of doubt with respect to empirical knowledge claims. The philosophical skeptic maintains that we do not know that we are justified in claiming to have empirical knowledge. It is easy to see that this view is wrong. One counterexample is logically sufficient to refute the skeptic. We do know that Socrates was mortal and we do know that we know this. A justification for what I have said is this: All men are mortal and Socrates was a man. Therefore, Socrates was mortal. We have presented a justification. Does the justification justify? Yes. The reasoning is correct and the premises are true. But do we know that the premises are true? That is an entirely different question, to which we will presently give an answer. But before discussing that question, we may note that we have already given a justification which does justify. Now the question may be raised whether we know that the justification justifies. Here we are invited to present a justification of the claim to know the premises of the preceding syllogism. This justification will be a probability argument, perhaps. The details need not detain us. But the argument will be cogent and will have true premises. This probability argument will justify our claim to know that all men are mortal and that Socrates was a man. I do not say that we know that this argument justifies a knowledge claim. I say only that it *does* justify the claim. And therefore we go on to say not only that we know that Socrates was mortal but also that we know that we know that Socrates was mortal. However, it is not here submitted that we *know* that we know that we know that Socrates was mortal. For the presupposition of a probability argument is an expectation which is not empirical knowledge at all. The cornerstone of skepticism is the proposition that one must know the truth of the premises and presuppositions of every justification which justifies an empirical knowledge claim. This dogma should be rejected by anyone who seeks, or claims to possess, empirical knowledge.

ARTHUR SMULLYAN

University of Washington

DISCUSSION

PHYSICS AND STRUCTURE

IN AN article in this journal,[1] William C. Clement claims to have refuted Russell's argument for his so-called "structuralist thesis" and moreover to have shown that the thesis is, in fact, probably false. In this paper I shall examine Clement's arguments and attempt to show that they are not valid. I shall not defend the structuralist thesis, but I shall seek to clarify it and thereby to dispel the plausibility of certain common objections to the thesis.

The structuralist thesis states that physics can yield knowledge of only the structure of the physical world, never any knowledge of the nature of the entities which possess this structure. Russell specifies the meaning of "structure" by giving the following definition of "similarity of structure":

> We shall say that a class α ordered by the relation R has the same structure as a class β ordered by the relation S, if to every term in α some one term in β corresponds, and vice versa, and if when two terms in α have the relation R, then the corresponding terms in β have the relation S, and vice versa.[2]

As Russell points out, the extension of this definition to polyadic relations is immediate. It is worth noting that similarity of structure, as thus defined, is closely related to, but not identical with, another concept which Russell calls "similarity [or "likeness" or "isomorphism"] of relations" and which he defines as follows:

> We may define two relations P and Q as "similar," or as having "likeness," when there is a one-one relation S whose domain is the field of P and whose converse domain is the field of Q, and which is such that, if one term has the relation P to another, the correlate of the one has the relation Q to the correlate of the other, and *vice versa*.[3]

The difference in the two definitions lies in the fact that similarity of structure holds among *ordered* classes (i.e., among classes each of which is the field of an ordering relation),[4] while similarity of relations holds among *any* classes each of which is the field of a relation. In other

[1] "Russell's Structuralist Thesis," *Philosophical Review*, LXII (1953), 266-275.

[2] *Human Knowledge* (New York, 1948), p. 254.

[3] *Introduction to Mathematical Philosophy* (London, 1948), pp. 53-54.

[4] An "ordering relation" is any relation which is asymmetrical, transitive, and connected.

words, if the relations P and Q in the definition of similarity of relations are taken to be ordering relations, one obtains the definition of similarity of structure; thus similarity of structure is a special case of similarity of relations. Finally, it should be remembered that Russell sometimes adopts the more general of the two above definitions to explain what he means by "structure."[5] It seems probable that the account of "structure" in *Human Knowledge* is restricted to ordered classes because classes ordered by spatial and temporal relations are of central importance in physics.

Clement summarizes, and quotes, Russell's reasons for believing in his structuralist thesis in reasonably accurate fashion. Attention must be called, however, to two significant lapses in quotation—the first (see footnote 6) apparently due to a mere slip or typographical error; the second (see footnote 7), though intentional, somewhat misleading in its omission of a key sentence. Furthermore, Clement's interposed comments tend to distort the significance of what Russell is saying. I shall, therefore, begin by outlining Russell's argument in his own words.

[I.] When a number of structurally similar complex events are ranged about a center in regions not widely separated, it is usually the case that all belong to causal lines having their origin in an event of the same structure at the center.[6] [This is called the "structural principle (or postulate)" by Russell.]

[II.] A "causal line," as I wish to define the term, is a temporal series of events so related that, given some of them, something can be inferred about the others whatever may be happening elsewhere. A causal line may always be regarded as the persistence of something—a person, a table, a photon, or what not. Throughout a given causal line, there may be constancy of quality, constancy of structure, or gradual change in either, but not sudden change of any considerable magnitude.[7]

[5] Cf., e.g., *Introduction to Mathematical Philosophy*, pp. 60-61.

[6] *Human Knowledge*, p. 492. Clement's quotation of this passage omits a crucial word, "complex." As I shall indicate below (p. 374), Russell's introduction of such criteria as complexity of structure to evaluate the probability of inferences based on the structural principle implies that, contrary to what Clement says, one does not determine solely by direct observation when the structural principle applies.

[7] *Ibid.*, p. 459. An example of a single causal line is the "process from speaker to listener in broadcasting" (*ibid.*). Clement omits the second sentence when citing the passage in the text above, thereby failing to convey Russell's view of the intimate connection between *persistence* and *causation*. Thus Clement writes: "Whether or not two entities have the same structure has nothing to do with their causal relations, if any: they have the same structure simply and solely on the condition that they meet the requirements set forth in the definition [of structural similarity]" (*op. cit.*, p. 269).

[III.] Natural processes are of two kinds. On the one hand there are those characterized by some form of *persistence*; on the other hand there are processes of synthesis or dissolution. . . . In synthesis and dissolution structure changes; in persistence structure remains in some degree constant.

The [structural] principle . . . has to do only with persistence.[8]

[IV.] From the standpoint of theory of knowledge, the most important application of our principle is to the relation between perception and physical objects. Our principle implies that in circumstances which occur frequently but not invariably, the structure of a percept is the same as that of each of a series of occurrences leading backward in time to an original occurrence, before which there were not spatio-temporally connected events having the structure in question.[9]

Clement claims (apparently assuming that Russell agrees with him) that one can never know, except by direct empirical means, (1) when a given series of events constitutes a causal line, or (2) when the origin of a causal line has the same structure as the events which make up the line, or (3) whether or not the perceptual process conforms to the structural principle. He then holds that in fact, since the perceptual process is generally *not* characterized by persistence, the conclusion IV is false. Now, even a cursory reading of Russell's *Human Knowledge* should reveal that, in the philosophically important cases, there can be no direct empirical test of *any* of the three inferences just mentioned. It is for the very purpose of justifying such inferences that the structural postulate and several similar postulates are introduced. But Clement seems unaware of this; or, at least, he gives no indication of the extent to which he is distorting Russell's views by insisting on direct empirical evidence for the propositions in question. In general, Clement completely ignores Russell's conception of the status of his postulates of scientific inference, namely, that these postulates "are the minimum assumptions, anterior to experience, that are required to justify us in inferring laws from a collection of data"; and that "scientific inference requires, for its validity, principles which experience cannot render even probable."[10] Before pursuing this point any further, I should like to restate Russell's argument in its essential form, eliminating a superfluous step and introducing an obviously presupposed premise.

[8] *Human Knowledge*, p. 473. "Persistence is illustrated by 'things,' light rays, and sound waves. Synthesis is illustrated by the presumed building up of heavier elements from hydrogen, by chemical combination, and by fertilization. Dissolution is illustrated by radioactivity, chemical analysis, and decay of an animal body after death" (*ibid.*).

[9] *Ibid.*, pp. 473-474. [10] *Ibid.*, p. xv.

I′. If several events are (a) structurally similar, (b) ranged about a center, and (c) near one another, then usually these events will belong to (independent) sequences of structurally similar events with a common origin at the center.

II′. A percept frequently belongs to a class of percepts satisfying conditions (a), (b), and (c) in I′.

III′. . . . A percept frequently belongs to a sequence of structurally similar events, whose origin is also the origin of many other sequences of structurally similar events, each issuing in a percept structurally similar to the given percept.

In the above formulation I have absorbed the definition of "causal line" into the statement of the structural principle; omitted the reference to persistence altogether (since the statement that the structural principle applies only to processes characterized by persistence is simply a way of calling attention to the special kind of natural process described in the consequent of I or I′); and added the obviously necessary premise (which, for Russell, is also obviously true) that our percepts do often satisfy the three conditions in the antecedent I or I′.

We can see now that the structural principle is of most utility when there are grounds for asserting that the members of a given set of events satisfy conditions (a), (b), and (c) in I′. As II′ indicates, the commonest grounds for such an assertion are the direct deliverances of sense perception. While it is quite possible (and, in fact, a frequent occurrence in the advanced sciences) for the grounds in question to be derived from established empirical laws, clearly the epistemologically primary application of the structural principle is when the events ranged about a center are percepts. Thus Russell says, "Our postulates are concerned more with the beginnings of science than with its advanced results."[11]

Furthermore, Russell mentions three factors which may serve to increase the probability of an inference based on the structural principle (which, incidentally, is inconsistent with Clement's idea that one must determine by direct empirical means when the principle applies). These factors are the following: (1) greater complexity of the observed structure, (2) larger number of observed instances of the given structure, (3) greater regularity in the grouping about a center. Russell remarks that these three factors may be implicit in the structural postulate as stated but that he has not succeeded in demonstrating this.

Although Russell's postulates of scientific inference are not sus-

[11] *Ibid.*, p. 477.

ceptible of direct empirical test, there is the indirect verdict of experience, namely, the consistency of these postulates with the conclusions derived by assuming their truth. In fact, this is the third of three requirements which Russell believes any postulate of inductive inference must satisfy:

> On the one hand, the postulate or postulates must be sufficient, from a purely logical point of view, to do the work that is asked of them. On the other hand—and this is the more difficult requirement—they must be such that some inferences which depend upon them for their validity are, to common sense, more or less unquestionable. . . . There must, of course, be no positive grounds for regarding a suggested postulate as false. In particular, it should be self-confirmatory, not self-refuting; i.e., inductions which assume it should have conclusions consistent with it.[12]

For example, we may ask whether the account of perception given by physics is consistent with the inferences by means of which physics is derived from percepts. Thus, if I see several objects in my perceptual space, am I justified in inferring (as I automatically tend to do) the separateness of the corresponding physical objects in physical space? According to Russell, the theory of light assures me that I am indeed justified, since "Light waves emanating from a source will, in suitable circumstances, pursue their course practically unaffected by other light waves in the same region."[13]

Let us now examine Clement's claim that the perceptual process is not characterized by persistence; that the structural principle is not, therefore, applicable to this process; and, consequently, that "the perceptual process is not of the type which will allow us to infer the structural features of the causes of our percepts."[14] In order to establish the crucial point in his argument, namely, the statement that the perceptual process is not characterized by persistence, Clement analyzes the perception of a flash of lightning (an example used by Russell) and concludes that neither (1) the flash itself, nor (2) the "complex interaction of physical and physiological factors at the retina," nor (3) the "brain event" is a process characterized by persistence. The first of these, says Clement, is clearly analogous to the explosion of a charge of dynamite, which Russell takes to be a process of dissolution; while the other two are judged to be processes not characterized by persistence apparently on the basis of their complexity alone (at least, Clement gives no other reasons). Since, then, the perceptual process contains at least some segments to which the structural

[12] *Ibid.*, pp. 438-439. [13] *Ibid.*, p. 206. [14] Clement, *op. cit.*, p. 272.

principle does not apply, Clement concludes that the principle cannot apply to the process as a whole.

Now, in the first place, it should be noted that the structural principle as stated by Russell is definitely not intended to be universally true (the phrase "it is usually the case" makes this sufficiently clear). Hence, even if some particular natural process should turn out to violate the structural principle, that fact in itself need not lead Russell to give up his principle. Furthermore, the very evidence for the existence of such an unusual natural process would, according to Russell, necessarily rely on the assumed truth of the principle in certain other natural processes (since practically all empirical evidence involves inferences, via the structural principle, from direct sense perception). This last point suggests the nature of the fallacy in Clement's attempt to demonstrate the failure of the structural principle when applied to the process of perception. For to say, for example, that a flash of lightning is not characterized by persistence of structure can only mean that *we infer from our perception of the flash* that its structure does not persist. But let us examine this flash of lightning more closely.

Looking at a particular part of the sky, we notice a flash of lightning; the flash is of very brief duration, perhaps a small fraction of a second. In what sense is this flash a process not characterized by persistence? To answer this question we must carefully distinguish between the *perceived* flash and the *inferred physical process* which we correlate with our perception of the flash.[15] To the eye, the flash is practically instantaneous—a single event, not a process at all—and hence the question of persistence does not even arise with respect to the flash itself. However, Russell's statement that the explosion of a charge of dynamite is a process not characterized by persistence suggests that we consider the flash of lightning from another point of view, namely, as a momentary event, severely localized in space and time, and preceded and followed by events of a very different structure (the "empty" sky). From this point of view, the total sequence of observed events: empty sky—lightning—empty sky, is not part of a single perceptual process at all. Rather, each stage of the sequence must be considered as the perceived effect of a new physical structure, and there is no reason to conclude that any of the perceptual processes involved violates the structural principle.

If we now consider the flash of lightning as a physical process, we note that all inferences as to the structure of this process (e.g., its

[15] Why and how this correlation is made are questions whose answers would involve a good deal of theoretical physics.

configuration in physical space) are necessarily based on the structural principle—together, of course, with any relevant accepted physical laws. One of these inferences would tell us of the spatiotemporal discontinuity in the electromagnetic field which serves to represent the flash of lightning. But such a lack of persisting structure in the electromagnetic field by no means implies any lack of persisting structure in the process involved in perceiving the flash. In fact, the very inference which leads to the conception of physical discontinuity depends upon the assumption that our percepts do give an accurate account of physical structure.[16]

With regard to the other parts of the perceptual process involved in observing a flash of lightning, it is clear that all of them will be present in *any* act of perception, so that the following considerations are of quite general applicability. Clement lists these parts of the perceptual process, in their proper temporal order, as follows: the transmission of the light rays through the intervening medium to the eye; the refraction of the light rays by the lens of the eye; the transmission of the light rays through the vitreous body of the eye; the complex interaction at the retina, producing varying electric potentials in the nerves leading from the retina to the brain; the transmission of these potentials to the brain; the complex event in the brain which constitutes the percept. According to Clement, the complex events occurring at the retina and in the brain are not processes characterized by persistence; nevertheless, Russell has been misled into believing otherwise by the fact that all the remaining processes *are* characterized by persistence. To clarify the issue we must, as before, distinguish *observed* from *inferred* structural similarity. Thus, when Russell mentions the explosion of a charge of dynamite, the passing of white light through a prism, and the falling of a drop of ink into a glass of water as examples of processes to which the structural principle does not apply, he must be thinking of observed structural similarity. Clearly, each of these three processes involves radical changes in *perceived* structure, but—and this is the crucial point—not necessarily any significant changes in the *physical* structures assumed to be involved in the processes. Precisely one of the great advantages of physical laws over common-sense laws lies in the more extensive persistence of structure according to the physical laws.

Russell is using the distinction between perceived and physical

[16] Not a completely accurate account, of course. Thus, for example, in the case of a flash of lightning, we may *seem* to see the lightning travel from the cloud to the earth, whereas we know from physics that often the actual flow of electricity is in the other direction.

structure when he says of the explosion of the dynamite charge that "all the structures involved are changed except the atoms,"[17] since atoms are physical structures with a high degree of persistence. Similarly, we should expect Russell to admit that the dispersion of white light as it passes through a prism involves little change in the physical structure of the light waves: according to the science of physical optics, there is simply a differential change in the directions of the light waves of different frequencies (which, however, is experienced by the eye as a fairly marked change in both quality and structure). Furthermore, there seems little reason to assume that the complex physiological interactions occurring at the retina and in the brain are not characterized by persistence of physical structure. It is no doubt true that at present we are unable to specify in detail just how, and how much, the structure of the light rays persists at the retina and in the brain; nevertheless, I question whether any scientist would expect to find a sudden discontinuity in physical structure at these critical junctures in the perceptual process.[18] In fact, such discontinuities would, in Russell's view, constitute lapses in causality which would just about make impossible *any* knowledge based on perception. Clement's discussion of this point, and indeed his attitude toward the structuralist thesis is general, is rather puzzling. To say, as he does, that the structuralist thesis is probably false means presumably that we can know *more* than the mere structure of the physical world; thus we find Clement arguing that to know anything about the structure one must also know some nonstructural features of the physical world. On the other hand, not only does Clement never explain just how these nonstructural features can be known, but he also insists that the perceptual process is such that one cannot validly infer anything about the structure of the physical world from what is given in sense perception.

[17] *Human Knowledge*, p. 473.

[18] The Gestalt psychologists, for example, would expect to find just such a persistence of structure. Thus, Köhler says: "all experienced order in space is a true representation of a corresponding order in the underlying dynamical context of physiological processes" (*Gestalt Psychology* [New York, 1929], p. 64). Russell himself points out some of the complexities of the perceptual process: "a constant stimulus does not produce a constant state of excitement in the nerve fibers, but a series of responses with quiescent periods between. . . . It is supposed that in the brain, or the spinal column, there is a converse mechanism which reconverts the discrete impulses into a continuous process, but this, so far, is purely hypothetical" (*Human Knowledge*, p. 38).

Once more, however, the very search for physiological laws characteristic of sensation implies a belief in the persistence of *some* structure.

In other words, whether one can know any nonstructural features of the physical world or not, it appears that no knowledge of physical structure can be based on sense perception. From the thesis that it is false that we can know *only* the structure of the physical world, Clement has passed to the thesis that we cannot know *even* the structure of the physical world. One is left wondering how, in Clement's view, any knowledge at all is possible.

I should perhaps point out that I am not claiming that *no* physical processes involve discontinuities of structure: Russell's example of atomic fission in the explosion of an atom bomb—in which, as Russell says, "even the atoms change"—may very well be such a process. To perception, the explosion of an atom bomb is clearly not a process characterized by persistence, and this may reflect accurately the randomness in the fission of the atoms and the disruption of their structures. On the other hand, the presumably slighter change in structure which occurs when dynamite explodes *appears* equally sudden and spontaneous. This brings out an important point: the structural principle does not lay down *necessary* conditions for an inference to physical causes but only *sufficient* conditions. In other words, even if the three conditions specified in the antecedent of the statement of the structural principle are not fulfilled as far as sense perception can judge, it may be possible to infer the existence of such physical causes by means of established physical laws. The normal development of physics has invariably led to increased reliance on physical laws for making inductive inferences; such a procedure need not be (and, according to Russell, has not been) inconsistent with the truth of the initial postulates of scientific inference.

Clement, in the article cited, not only doubts the validity of Russell's argument for his structuralist thesis but also questions the very meaningfulness of this thesis. Can we really know the structure of an object, Clement asks, without knowing something further about the nature of that object? More generally, one might ask: Is it possible to know a relation without knowing anything about the relata of that relation? Clement claims that Russell himself denies this possibility when he says, "To exhibit the structure of an object is to mention its parts and the ways in which they are interrelated";[19] for how can one "mention" the parts of an object without "knowing something" about these parts other than the fact that they bear certain relations to one another? Furthermore, how can one, on Russell's

[19] *Human Knowledge*, p. 250.

view, distinguish the various *distinct* objects or events with the same structure? Returning to our earlier example, how can one distinguish the flash of lightning from the light rays, or either of these from the brain events, if all three possess identical structures? As Clement says, "There are, in principle, an infinite number of possible entities which can have the same structure as a given percept, just as there are an infinite number of Kantian things-in-themselves which can be the causes of our percepts."[20]

There are two separate issues involved in the above criticism by Clement, neither of which, I believe, constitutes a genuine difficulty in the structuralist thesis. First we ask: Is is meaningful to speak of the structure of an object without knowing something about the object beyond its structure? That the answer to this question is affirmative should be evident enough from even the simplest statement of applied mathematics, such as "There are six things in this class." Such a statement asserts something about the cardinal number of a certain class but says nothing at all about the nature of the things which are members of that class. According to the Frege-Russell interpretation of cardinal numbers (which it seems appropriate to employ in elucidating Russell's structuralist thesis), the number six means simply the class of all sextuples (whose members, we shall assume for the sake of simplicity, are "concrete" things). Or, we may, if we like, think of "sixness" as the property possessed in common by all sextuples; such a property is obviously completely independent of the nature of the things which are members of any of the sextuples. The knowledge of the cardinal number of a class may perhaps, then, be thought of as a highly primitive kind of purely structural knowledge.[21] Admittedly, the cardinal numbers of the various classes of concrete things are not usually regarded as a very important part of our knowledge of the world. Nevertheless, if one holds that theoretical physics is, at least ideally, wholly mathematical in its formulation, then, since mathematics always deals with the structure (as defined above) of *some*

[20] Clement, *op. cit.*, p. 275.

[21] Strictly speaking, according to Russell's definition of similarity of structure (see p. 371), only *ordered* classes can be structurally similar; thus, Russell says: "It is to be observed that structure always involves relations: a mere class, as such, has no structure" (*Human Knowledge*, p. 254). However, for the purpose of illustrating structural knowledge by a very simple example, we may think of cardinality of classes as a degenerate form of structural similarity. More precisely, since a one-one correlation can always be set up between the members of two classes of the same cardinal number and since there are *no* ordering relations at all, the criterion of structural similarity is trivially satisfied.

classes and relations, it follows that theoretical physics gives us a purely structural knowledge of the world.

Consider geometry, for example. All geometrical properties and relations—such as number of dimensions, continuity, type of metric, etc.—are expressible as structural characteristics of the set of certain ordered classes of real numbers (namely, those ordered classes of real numbers which are ordinarily called "points"). The fact that geometry is purely structural in content explains why Russell can say, "The mathematician need not concern himself with the particular being or intrinsic nature of his points, lines, and planes, even when he is speculating as an *applied* mathematician."[22] However, the applied mathematician must, of course, be concerned with whether his space (what we have earlier been calling "physical space") is isomorphic with perceptual space, for:

> If the phenomenal world has three dimensions, so must the world behind phenomena; if the phenomenal world is Euclidean, so must the other be; and so on. In short, every proposition having a communicable significance must be true of both worlds or of neither: the only difference must lie in just that essence of individuality which always eludes words and baffles description, but which, for that very reason, is irrelevant to science.[23]

In Russell's view, then, mathematical propositions are about entities (classes and relations) which, while they can be designated or named for the purpose of distinguishing them one from another, cannot be—and need not be—described or characterized in qualitative (i.e., nonstructural) terms. Thus, "mentioning" the parts of an object does not require that we "know something" about these parts any more than designating the six members of a class by the letters a, b, c, d, e, f requires that we know something about these six members.

Now, assuming that it makes sense to speak of knowing the structure of an object without knowing the entities of which the object is composed, we must show how the members of a set of structurally similar objects can be distinguished from one another. First of all, we must carefully distinguish between *physical* objects or processes and the corresponding *observed* objects or processes. The flash of lightning at a spot in the sky distant relative to my eye: this is an observed pattern of events in my perceptual space and time. The stream of electrically

[22] *Introduction to Mathematical Philosophy*, p. 59.

[23] *Ibid.*, p. 61. In other writings (notably, *Human Knowledge*) Russell discusses the "essence of individuality" in considerable detail. I am not convinced by Russell's solution to the problem of individuality and hope to treat this matter elsewhere. In the present context it is sufficient to assume that *some* consistent means can be found for distinguishing individuals.

charged particles, the electromagnetic waves, the reaction of certain photosensitive compounds in the retina, the transmission of nerve impulses, the occurrence of events in the brain: this is a sequence of physical processes in physical space and time. Some, at least, of the physical processes may be (roughly) correlated with distinct percepts, so that the physical processes are thereby distinguished from one another in an indirect but empirically significant way. For example, by virtue of the fact that I can perceptually distinguish my brain (or at least my head) from a distant cloud, I can also distinguish the stream of electrically charged particles from the structurally similar events in my eye and brain. Thus, even if it turns out that the only cognizable type of physical structure consists in spatiotemporal relations, we are still left with the wealth of perceivable relations—spatiotemporal and otherwise—in our perceptual space-time for keeping similar physical structures distinct from one another.[24]

Perceivable relations, however, do not seem to be adequate for making all of the required distinctions among physical structures. For example, it is not easy to see how, in the above example, one might distinguish perceptually between the stream of electrically charged particles and the electromagnetic waves or, for that matter, between earlier and later stages in the electromagnetic disturbance itself. More generally, it may be said that perceptual diversity is not a necessary condition for diversity of physical events or processes (consider the indefinitely numerous mixtures of spectrally pure colored lights which appear white to the normal human eye). In fact, perceptual diversity does not even seem to be a sufficient condition of physical diversity: even apart from sensory illusions (e.g., a physical object may appear doubled under certain conditions), there is the thoroughly familiar case of a single physical object corresponding to a multiplicity of sensory objects (the seen apple, the touched apple, the tasted apple, etc.).

How, then, are similar physical structures to be distinguished from one another? Let us turn to the actual methods of theoretical physics for an answer. Earlier and later stages of a train of electromagnetic waves may be structurally similar insofar as velocity of propagation, wave length, amplitude, phase, and direction of polarization are

[24] "The astronomer's sun, for instance, is very different from what we see, but it must have a definition derived from the ostensive definition of the word 'sun' which we learned in childhood. Thus an empirical interpretation of a set of axioms, when complete, must always involve the use of terms which have an ostensive definition derived from sensible experience" (*Human Knowledge*, p. 242).

concerned; nevertheless, these same waves are structurally dissimilar—and hence identifiable as distinct physical structures—with respect to their spatial and temporal relations in some appropriate co-ordinate system. This brings out an extremely important point: the structure of a set of entities is not an absolute property of the set but rather is relative to the relation whereby the entities in the set are ordered.[25] Consequently, two sets of entities may be structurally similar in some respects but not in others. In terms of the definition of similarity of structure this means that the classes α and β may be ordered by the relations R and S respectively in such a way that the resulting ordered classes are structurally similar, but α and β may at the same time be ordered by the relations R' and S' respectively in such a way that the new ordered classes are *not* structurally similar. Some simple mathematical examples should make this clear. Consider the class of all integers. This class can be ordered in the following two ways (among others): 1, 2, 3 . . ., and 1, 3, 5 . . . 2, 4, 6 . . . . Now, these two ordered classes are not structurally similar (there is no one-one relation correlating the members of the two classes and preserving the order); therefore, any ordered class (such as the odd integers in the order 1, 3, 5 . . .) which is structurally similar to the class $\{1, 2, 3 \ldots\}$ cannot be structurally similar to the class $\{1, 3, 5 \ldots 2, 4, 6 \ldots\}$. In this example, then, we have found two classes (the class of all integers and the class of odd integers) which are both structurally similar and dissimilar depending upon which ordering relation we choose. It must be emphasized that all such orderings are equally legitimate or "natural." The integers *really are* characterized by the structural properties exhibited by the different possible ordering relations.[26]

Geometry abounds in entities which are structurally similar in some respects and structurally dissimilar in others: figures which are topologically equivalent need not be metrically equivalent (e.g., a circle and an ellipse); figures which are projectively equivalent need not be metrically equivalent (e.g., an isosceles triangle and a scalene triangle). In each of these cases one is dealing with a well-defined class of transformations which preserve some structural properties of geometrical figures but not others.

[25] As Russell puts it: "Out of the terms of a given class many structures can be made, just as many different sorts of houses can be made out of a given heap of bricks" (*ibid.*, p. 254). Cf. *Introduction to Mathematical Philosophy*, p. 31: "The fact that a class may have many orders is due to the fact that there can be many relations holding among the members of one single class."

[26] Cf. Russell's remarks on this point: *Introduction to Mathematical Philosophy*, p. 30.

If it be objected that in our above examples a knowledge of the members of the two classes of integers or of the distinctions among various types of geometrical figures was tacitly presupposed, I reply that such knowledge is itself purely structural in character (thus the integers may be implicitly defined by Peano's axioms or explicitly defined by the method of Frege and Russell). Furthermore, our mathematical examples have an immediate relevance for theoretical physics, if, as Russell claims, that science deals very largely in the abstract entities of pure mathematics (always remembering, of course, that at least some of the entities of theoretical physics must ultimately be correlated with observable qualities and relations).

The above considerations seem to me to resolve Clement's dilemma:

> ...if we significantly distinguish between such things as brain events, light waves, and flashes of lightning, then these things either do not have the same structure (as has been supposed), or . . . we know more about them than their structural properties. There is no escape from this argument.[27]

In the first place, Clement uses the word "things" equivocally: on the one hand, there are the isomorphic perceived entities which we *can* "significantly distinguish" on the basis of their qualitative differences; on the other hand, there are the isomorphic physical entities possessing no known qualitative differences but which, in some cases at least, may be distinguished by means of their correlation with the perceived entities. In short, sometimes one can distinguish the mere multiplicity of the physical entities by means of the observed differences in the correlative perceived entities[28]—without, of course, giving up the assumption that the physical entities possess the same structure.

In the second place, Clement uses the term "structure" as if a thing always had a single perfectly definite, or absolute, structure. Since, as we have seen, the structure of a thing is relative to the ordering relation selected from among all those relations characteristic of the constituents of the thing, it follows that two isomorphic things may sometimes be significantly distinguished by certain of their non-isomorphic structural properties. And, indeed, it would seem that this method for distinguishing isomorphic entities is precisely that used in theoretical physics. (Clement rejects precisely this method, *op. cit.*, p. 274).[29]

ROBERT PALTER

University of Chicago

[27] Clement, *op. cit.*, p. 274.

[28] This correlation is effected by ostensive definitions. Cf. n. 24.

[29] I am grateful to Professor Howard Stein for several stimulating and helpful discussions on some of the issues in this paper.

REGULAR RULES OF INDUCTION

IN *Problems of Analysis*,[1] Max Black formulates and discusses what he calls "a counter-inductive policy." Essentially the policy amounts to this: Given a sequence of events defined by the property A, given a sample of this sequence having n members, and given that m members of the sample have been found to have the property B, predict that $\frac{n-m}{n}$ of the total sequence of A's will be B's. The person who adopts this policy is "a congenital pessimist," for he believes that the ratio of B's to A's in the total population is exactly the ratio of non-B's to A's in the observed sample. Furthermore, "When most A's have been B, he expects the next A *not* to be a B; and the more A's have been B in the past, the more firmly he anticipates that the next A will *not* be a B." Black subsequently remarks, "I have no wish to suggest that a 'counter-inductionist' would be behaving rationally (though anybody who could explain in convincing detail why this should be so would have mastered the most important problems in the philosophy of induction)." In this note I shall attempt to show that the counter-inductive policy involves an "irregular rule of induction" and that rules of this class are unsatisfactory because they lead to formal contradiction.

Suppose we have a sequence of A's and three mutually exclusive attributes, B_1, B_2, and B_3. In an observed sample of this sequence let us suppose 5/8 of the elements have the attribute B_1, 1/4 have the attribute B_2, and 1/8 have the attribute B_3. Now the counter-inductionist must prefer B_3 to the other two attributes, for he predicts that 7/8 of the A's are B_3. But consider the attribute non-A which obviously is not exemplified in the sample. He should conclude that all A's are non-A's, for that is the consequence of the counter-inductive rule. The same may be said for every attribute which fails to be exemplified in the sample, even though many such attributes will be mutually incompatible. Thus, if the population is balls in an urn, and the sample contains red, white, and black balls only, the counter-inductionist would be required to predict that all remaining balls are green, *and* that all remaining balls are blue, *and* that all remaining balls are pink, and so on.

Surely the counter-inductionist must avoid troubles of the foregoing sort. So let him hold to the counter-inductive policy for only those attributes which are actually exemplified in the sample—i.e., attri-

[1] Ithaca, N.Y., 1954, pp. 171-173.

butes for which $m > 0$. In this case, he is forced to conclude that 7/8 of the A's are B_3, that 3/4 of the A's are B_2, and that 3/8 of the A's are B_1. B_1, B_2, and B_3 are mutually exclusive; therefore, he must conclude that 16/8 of the A's have one or another of these attributes. For larger sets of mutually exclusive and exhaustive attributes the situation becomes even worse.

We might try to save our counter-inductionist from contradiction by recommending that he deal with dichotomous classifications only. Thus, if he asks about B_1 he will regard the other two attributes as constituting non-B_1. Then, he will predict that 3/8 of the A's are B_1 and 5/8 are non-B_1. Following the counter-inductive policy, he must predict that the next A will be a non-B_1. But let him never ask whether it will be a B_2 or a B_3; if he does he will find out that it must be *both* in spite of the fact that the two attributes are mutually exclusive. Further, let us hope that groups of counter-inductionists never meet together. For if a second counter-inductionist is curious about the same sample and population, but about a different attribute, say B_2, he will make a different dichotomy and find that 3/4 of the A's are B_2 and 1/4 are non-B_2. According to our second counter-inductionist the next A must be a B_2. If a third counter-inductionist, who is interested in the same sample and population but in the attribute B_3, appears, he will dichotomize B_3 and non-B_3. He will conclude that the former attribute is possessed by 7/8 of the population and the latter by 1/8. He will predict that the next A must be a B_3, a result which contradicts his predecessor, the second counter-inductionist.

Finally, further dire consequences occur if all three counter-inductionists compare notes. Non-B_1 is equivalent to the logical sum of B_2 and B_3. But the first counter-inductionist has found that non-B_1 is possessed by 5/8 of the total population, whereas our second and third counter-inductionists together conclude that it must be possessed by 13/8 of the total population, a result which not only contradicts the conclusion of our first counter-inductionist but is paradoxical enough in its own right.

These difficulties in the counter-inductive policy occur because it involves a rule of inductive inference which violates an obvious necessary condition for satisfactory rules. This condition can be formulated as follows:

> *Condition I:* Given (1) a sequence $\{S_i\}$ of events defined by the attribute A, (2) a set of properties $B_1 \ldots, B_k$ which are mutually exclusive and exhaustive within $\{S_i\}$ and (3) a sample of n members of $\{S_i\}$, m_j of which have the property $B_j (1 \leq j \leq k)$;

then, any rule R for making estimates E_j of the probability $P(A, B_j)$ or of the relative frequency $F(A,B_j)$ of B_j within the total sequence $\{S_i\}$ must be such that

$$\sum_{j=1}^{k} E_j = 1.$$

That any rule which violates this condition will lead to contradiction is shown by the fact that, since $A \subset B_1 \cup \ldots \cup B_k$, it follows by the calculus of probability that

$$P(A,B_1 \vee \ldots \vee B_k) = 1.$$

Similarly, by ordinary arithmetic it follows that

$$F(A,B_1 \vee \ldots \vee B_k) = 1.$$

Also, by the calculus of probabilities,

$$P(A,B_1 \vee \ldots \vee B_k) = \sum_{j=1}^{k} P(A,B_j) = \sum_{j=1}^{k} E_j;$$

by arithmetic,

$$F(A,B_1 \vee \ldots \vee B_k) = \sum_{j=1}^{k} F(A,B_j) = \sum_{j=1}^{k} E_j.$$

If $\{S_i\}$ is an infinite sequence, statements above about the relative frequency of an attribute within the total population will have to be reformulated in terms of the limit of relative frequencies, but the foregoing remarks hold for the transition to the limit.

If we now add another obvious necessary condition for the satisfactoriness of an inductive rule:

> *Condition II:* Under the circumstances detailed in Condition I, E_j must never be negative,

then we have conditions analogous to those used by Carnap for the specification of regular m-functions and regular c-functions.[2] To call attention to the analogy, let us call a rule of induction R "regular" if it fulfills the foregoing conditions I and II. Irregular rules are uniformly undesirable because they can be used to establish contradictory results concerning a single sequence on the basis of a single sample.

Discussions of other irregular rules of induction are to be found in the literature. For example, Reichenbach has pointed out that his

[2] Rudolf Carnap, *Logical Foundations of Probability* (Chicago, 1950), pp. 294-295.

pragmatic justification of induction justifies an infinite class of "asymptotic rules" rather than any single rule.[3] He speaks of these rules as being "equally justified." Given a sequence of A's and given a sample containing n members, m_j of which are B_j, an asymptotic rule is a rule which says to predict that the limit of the relative frequency of B_j among A's is $\frac{m_j}{n} + c_n$, where c_n is some function which converges to zero as n tends to infinity. If c_n is a function of n only and is not always zero, then the asymptotic rule so defined will be irregular and can be rejected as inadequate for that reason. For example, if the rule is to predict that the limit of the relative frequency is $\frac{m_j}{n+a}$, where a is some positive constant, we have such an irregular asymptotic rule. For, in such a case,

$$\sum_{j=1}^{k} \frac{m_j}{n+a} = \frac{n}{n+a} \neq 1.$$

In general, if c_n is a function of n alone, we have the unsatisfactory result that

$$\sum_{j=1}^{k} \left(\frac{m_j}{n} + c_n\right) = 1 + kc_n.$$

It would be a mistake to suppose that the conditions of regularity will be sufficient to select one unique rule of induction; there are infinitely many regular rules. I am merely concerned to show that a class of rules widely discussed in the literature can be eliminated on formal considerations alone. In particular, the counter-inductionist can be convicted of hopeless irrationality without any penetration into the subtleties of the philosophy of induction. The problem of induction —the problem of selection among consistent inductive rules—remains.

WESLEY C. SALMON

Brown University

[3] Hans Reichenbach, *Experience and Prediction* (Chicago, 1938), pp. 354-355, and *The Theory of Probability*, 2d ed. (Berkeley, Calif., 1949), p. 447.

ON BEING IDEAL

1. Many philosophers in the past few decades have been exercised over the question whether or how ethical expressions could be "meaningful." In particular, many who accepted some form of verifiability criterion of meaningfulness have thought that either ethical judgments can have only "emotive meaning" or they must be a subclass of empirical assertions. It is to be expected, then, that those empiricists who were dissatisfied with the emotive theory should try to show that, in fact, ethical judgments are empirical statements of some kind or other, i.e., that sentences containing the typical ethical predicates ("right," "wrong," "evil," "just," and so on) can each be translated into empirical sentences which do not contain any ethical predicates. I do not intend here to take any stand on the reasoning I have just sketched, which has led people to attempt such translations or to think that they must be possible. What I do intend to do is criticize one program for making such translations; and if my argument is sound, I think it tends to show that no such program can succeed.

2. The particular proposal which I will criticize is advanced by Professor Roderick Firth,[1] who claims that ethical judgments may be construed as statements of empirical (if highly theoretical) psychology —as statements about certain possible attitutdes of an hypothetical "ideal observer," who is to be characterized without the use of any ethical or evaluative terms. I think the rationale of Firth's proposal can be expressed in the following way. Our ethical judgments are intimately related to certain of our psychological dispositions. For instance (although Firth does not offer any sample analyses which are this specific), my judgment that some kind of conduct is wrong is certainly related to a disposition on my part to disapprove[2] of that kind of conduct. But it would be incorrect to hold that when I say

[1] "Ethical Absolutism and the Ideal Observer," *Philosophy and Phenomenological Research*, XII (March 1952).

[2] I want "approval" and related words to be understood simply as psychological terms throughout this paper—as descriptive of people's feelings and attitudes. I do not think these words are commonly used to do just this job of reporting: more often, especially in the first person, they are probably used for taking a stand on a moral issue. But it is convenient to have a single word to describe the psychological states normally associated with moral judgments, and "approval" has been used for this purpose by other ethicists, notably by C. L. Stevenson, in *Ethics and Language*. (Without the preposition, the verb is often used, of course, for *doing* something rather than reporting on it, as in "I approve your application").

something is wrong, I am saying merely that I have this psychological disposition. We can see that this would be incorrect when we realize that I am quite willing to admit that I may sometimes approve of things which are really wrong—things of which I might well *dis*-approve if I were more fully informed about the relevant empirical facts, or if I were not blinded by self-interest, or if I were more sensitive to the feelings of other people, and so on. My attitudes of approval and disapproval, then, are not even infallible *guides* to right and wrong, and I realize the fact; so statements about those attitudes a fortiori cannot be taken as definitive of the *meaning* of my ethical judgments. But the interesting thing for present purposes is that we recognize certain specific factors as tending to make our reactions bad guides to moral issues, where those factors apply: we recognize that we are more likely to disapprove of something which is really right, for instance, in those cases where we are ill informed about the (nonethical) facts, or tempted by the hope of personal gain, or insensitive to the feelings of other people, or swayed by personal friendship or animosity toward the people involved, and so on.

3. And now it begins to look plausible to say that what is *really* morally wrong is what I *would* disapprove of if I were adequately informed, free of personal prejudices, and so on: in short, if I were free of all those factors which we could specify as tending to make me react inappropriately. It might seem plausible also to assume that the more we eliminated such distorting factors, the more closely would correspond the reactions of any two people: so that what *I* would disapprove of if all those factors could be removed, would be identical with what *you* would disapprove of if all the distorting factors in your case could be removed—would be identical indeed with what *anyone* would disapprove of who was free of these distorting factors. (I say it "might seem plausible to assume" this—in fact, it could be treated not as an assumption but as a logical requirement on our list of "distorting factors." This would be to decide against ethical relativism—in one clear sense of that term—by definition.)

4. The foregoing seems to me to be a fair account of the kind of reasoning which might lead one to adopt Firth's position, although my account is only partly based on what he explicitly says in his article. What he does, in any case, can be reconstructed in the following way (I presume to "reconstruct" Firth's article, not because it is not constructed with exemplary clarity in the first place, but simply because I find a different order of exposition more convenient for my purposes). (a) He considers what kinds of things we recognize as disqualifying,

or to use my term again, as tending to distort our reactions (e.g., ignorance, personal emotional involvement with people concerned in a given case where ethical judgment is to be made, and so on); (b) he *defines* an "ideal observer" as one who possesses a certain list of empirically describable characteristics such that, having those characteristics, he could not suffer from the disabilities mentioned in the previous list; (c) he offers the following schema for the definition of ethical predicates: "*x* is *P*" where "*P*" is some ethical predicate, is identical in meaning with some statement of the form "An ideal observer (as defined) would react to *x* in such-and-such ways." It will be convenient for me to work with a particular example of this schema, e.g., " '*x* is wrong' means 'An ideal observer would disapprove of *x*.' " This is not Firth's example; he does not offer detailed examples.

5. Now Firth recognizes perfectly well that "ideal observer" cannot be defined in part as "one who disapproves of whatever is wrong, and approves of whatever is right"; nor can "ideal observer" be defined as "one who is free of all those factors that tend to distort our moral reactions." If his program is to succeed—i.e., if he is to eliminate peculiarly ethical expressions in favor of those which designate empirical properties—there must be no ethical terms in the definition of the ideal observer. Firth claims that he is not smuggling any ethical notions into his definition in any such way: rather he first states *in nonnormative terms* certain criteria (which are, he believes, the criteria an observer would have to satisfy in order to be free of disqualifying, or distorting factors); and he then *defines* the ideal observer as one who satisfies those criteria. Now of course the *reason* for choosing just those criteria is that Firth thinks that anyone who satisfied them would react favorably to anything which was morally good, or right, and so on, and unfavorably to anything which was morally wrong, evil, and so on; but the fact that this is the *reason* for the choice does not in any way appear in the definitions which would satisfy his schema. The schema for definition of ethical terms (see my preceding paragraph) has no normative terms in the definiens.

6. It will be helpful to distinguish (as Firth does—see especially pp. 330-331) between Firth's *program* and the particular list of *criteria* he employs. By his *program* I mean the project of defining ethical predicates in terms of the psychological dispositions of an hypothetical ideal observer, where the ideal observer is defined by a specific list of nonnormative criteria. One might, of course, agree with Firth on the feasibility of this program but disagree about his particular list of criteria. The criteria he chooses are as follows: (a) the ideal observer

is omniscient with respect to nonethical facts; (b) he is omnipercipient (i.e., he is able to visualize all aspects of actual and possible situations, including notably the feelings of the participants); (c) he is disinterested, in the sense that he has no interests in any *essentially particular* aspects of any situation or person; (d) he is dispassionate, in the sense that he is not swayed by emotions dependent upon any essentially particular properties of a situation or person; (e) he is consistent (a requirement which, as Firth points out, will be fulfilled if we have properly enumerated the other properties relevant to his ethically significant reactions); (f) in other respects he is normal.

7. Mr. Firth makes a good case for the choice of just these characteristics: I am afraid that my bare list of them, without his accompanying discussion, is unfair to him, but of course my reader can turn to his article to rectify this injustice. I will not discuss this list of criteria, because my criticism is much more radical. I shall argue that the program is wrong in principle: that Firth's stipulations defining "ideal observer" represent a moral choice and can only be defended as such; and that if the *program* were sound, it would be impossible—logically impossible—to conduct a defense of the choice of a particular set of criteria. And since I think that even if one could accept the feasibility of the program, a legitimate question *could* arise as to the correctness of any given set of criteria, I conclude that the program itself is radically at fault.

8. I suppose it might be granted without argument that a legitimate question might arise as to the correctness of a given set of criteria. Firth says, for instance:

> Any plausible formulation (of the required definitions) is certain to be very complex, and there is no reason to suppose that philosophers could ever reach complete agreement concerning all the details of an adequate analysis. But this in itself should not prevent philosophers from agreeing that this general *form* of analysis is valid. Nor would it necessarily be irrational for a philosopher to decide that this general form is valid, although he is dissatisfied even with *his own* attempts to formulate a concrete analysis [*op. cit.*, p. 330].

And he speaks more explicitly to my point:

> Any such analysis, if it is at all plausible, is certain to assign a number of complex characteristics to an ideal observer, and to refer to complex psychological phenomena in describing the nature of his ethically-significant reactions. And assuming that ethical statements *can* be analyzed in this manner, there is no good reason to believe that all human beings, no matter what the extent of their individual development, and no matter what their past social environment, could analyze their ethical statements correctly by reference to precisely the same kind of ideal observer and precisely the same psychological phenomena [*ibid.*].

Thus I hardly need argue (at least with Firth) the point that a defense of one set of criteria as against another might be necessary. Not only might we have forgotten some criterion which we should want to include if we thought of it, but it might well be that there were other determinants of a person's ethical attitudes,[3] of which we might be ignorant—and which, in any case, we could not attribute to the ideal observer without begging some genuine moral question. I suppose that Firth would admit that there are probably determinants of our attitudes which neither he nor I knows about; but the crucial question is whether there could be any such determinants which would not be "smoothed out," so to speak, by the very liberal endowments of Firth's ideal observer. What bothers me is that I can see no reason to suppose that an observer of the sort Firth mentions would approve, say, of people's being happy, other things being equal, rather than of their being unhappy; and yet, if the ideal observer's reactions were to determine the very meanings of ethical predicates, we should surely have to constitute him so that he would take a stand on happiness. Or if it seems too bizarre to suppose that he might disapprove of happiness, might it not be that he would disapprove of laughter on the part of statesmen and philosophers (cf. *Republic*)? Or if this seems too jocular, can we be sure that Firth's criteria are sufficient to determine the ideal observer's attitude toward sexual enjoyment of any kind? Or toward the marriage of cousins? Or toward the moral aspects of the graduated income tax? To be sure, the ideal observer would be fully informed on all the consequences of various social practices, and so on, he would know exactly how it felt to be penalized or rewarded in certain ways, he would not tend to favor any one group over another for any sort of egocentric reason—but can we go on to say that he has been fully enough defined to insure that he would even give a hang about all the important moral questions?

9. Now I have been complaining that, for all we know, Firth's criteria might not be sufficient to determine the morally relevant reactions of the ideal observer on all the interesting moral questions; but it looks to me as if it would be even worse if he *were* defined sufficiently to determine his reactions to all these things. Because if someone had defined the ideal observer by a list of criteria sufficient to determine *all* his ethically significant reactions, he would thereby have resolved all outstanding substantive questions of ethics by definition

[3] See Professor R. B. Brandt's remarks in "The Definition of an 'Ideal Observer' Theory in Ethics," *Philosophy and Phenomenological Research*, XV (March 1955), 408-409.

—by his definition of "ideal observer."[4] And if someone could show us that his list of criteria *was* sufficient to determine the reactions of his ideal observer on a disputed ethical point, and if we disagreed with the verdict of this definition, we might very well challenge his list of criteria for being an ideal observer. For what, after all, was the basis on which the criteria were selected in the first place? Simply this: that we believe that a person tends to react appropriately to morally significant situations, insofar as he approximates the satisfaction of these criteria. So one of the tests of the selection of the criteria must be that an observer who satisfied them *would* react appropriately, and thus an argument as to which of two incompatible criteria should be taken as definitive of the ideal observer (if these two suggested criteria were expected to lead to different reactions) would be, at bottom, a moral argument.

10. I am arguing (a) that Firth's program would fail if the ideal observer were *not* sufficiently characterized to insure that he *had* ethically significant reactions toward everything which is ethically significant; and (b) that it would be even worse if he *were* so characterized. My reason for (a) is that, according to the analysis under consideration, to make any moral judgment is just to make a statement about the reactions of the hypothetical ideal observer: so whatever he failed to react to would have to be ethically neutral (or perhaps would fall outside the range of significance of ethical predicates). But of course if many things which should turn out to be ethically neutral according to the analysis did *not* strike us as ethically neutral in our preanalytic estimates, we should have to reject the analysis. My reason for (b) is that the specification of any criterion which helps define the ideal observer is really a move in a moral argument, although its status in Firth's program is officially merely that of a partial definition of the meaning of some ethical predicate: and it would be highly arbitrary to "settle" any genuine moral question simply by the definition of some ethical predicate.

11. But if I am right so far, an even worse consequence seems to follow for Firth's program. I want now to support my contention of paragraph 7 that if his program were sound, it would be *logically*

[4] In fact, he would not have settled all substantive ethical questions until he had also offered a highly confirmed psychological theory of sufficient complexity and detail. But my point is just that all the *moral* work and all the *philosophical* work would be done if the ideal observer were characterized in sufficient detail: all that would remain would be for psychologists to crank out the moral answers from the framework provided by the philosopher.

impossible to defend one's choice of any criterion which was disputed. My point can be stated very briefly. An argument as to the characteristics of the ideal observer is a step in the *definition* of ethical predicates; but if I am right that the defense of one criterion rather than any incompatible one is a *moral* argument, then any such argument is a moral argument about the meaning of moral terms, including ultimately[5] those in which it is being carried on. It seems to me, then, that any such argument would be unsettleable, because it would have to be carried on in terms which remain ambiguous—and ambiguous in just those respects which would prevent it from being settled—until it was settled. Moreover, not only would an argument about the characteristics of the ideal observer be logically impossible to conduct if the program were sound: any *moral* disagreement between people who defined their ideal observers differently[6] would be logically impossible. For instance, let us suppose that I tried to argue with a Nietzchean on the subject of the graduated income tax. If Firth's program is sound, then if my opponent said that a graduated income tax is an evil thing, he would mean something like "An observer of characteristics *C* would find the graduated income tax revolting"; while if I said that such a tax was on the whole a good thing, I would mean something like "An observer of characteristics *D* would, on the whole, find the tax heart-warming." Now clearly, if these two classes of characteristics, *C* and *D*, were not identical, the Nietzchean and I might both be right; but equally clearly, we *meant* to disagree. My point, of course, is not merely that we could not *settle* the moral question involved: we could not even argue it. We could neither agree nor disagree about it.

[5] One might object that perhaps the (moral) argument about the meaning of any particular moral predicate could be carried on without using that particular predicate, so that no circularity would be involved; but of course this reply would not do. It would not do because Firth's problem is that of characterizing the kind of meaning which is peculiar to ethical predicates in general, and not just of defining some subset of ethical predicates.

[6] I am not merely making a joke of a kind which can be made whenever a complicated philosophical analysis is proposed, when I speak of arguments between people who define their ideal observers differently. It is in no way crucial to Firth's proposal or to my objections that most people engaged in moral argument do not have an explicit theory as to the meaningfulness of their ethical predicates. What I describe (for brevity's sake) as "how someone defines his ideal observer" is implicit in the appeals he would make, if pressed far enough, to certain kinds of rules or criteria—kinds related to those to which Firth resorted in designing his analysis.

12. Now it is sometimes possible, where disputants find that they are using some central word in their argument differently, for them to settle the meaning of that word and then return to conduct their argument more profitably. But that could not happen in the case under consideration. If we found out that we were in effect defining our ideal observers differently, we might indeed turn to the question how he should be defined, and try to settle it first. But as I have tried to show in paragraph 11, this would be impossible. Not only could we not *settle* the question; here again we could not even disagree about it For I should now be saying, "An observer of characteristics *D* would approve of the ideal observer's being defined by characteristics *D*"; while my opponent would be saying, "An observer of characteristcs *C* would approve of the ideal observer's being defined by characteristics *C*." Here again, of course, we might both be right; but we had intended to *argue* with each other.

13. I think the issue between Mr.Firth and me can be sharpened by consideration of an analogy which he accepts and I reject. Recognizing the impossibility of an adequate characterization of "normality" (his sixth requirement as to the nature of the ideal observer), he contends that the difficulties involved are "practical rather than theoretical" and that they do not affect his thesis. He continues: "There are analogous difficulties, moreover, in formulating a dispositional analysis of the statement 'This is (really) yellow'; and I have yet to find any convincing reason, indeed, for believing that 'yellow' can be defined dispositionally although 'right' cannot" (p. 345). I shall not deal explicitly with the normality of the observer in the two cases, but with the more general question of the justification of the choice of *any* of the standard conditions in each case; and it seems to me that we can thus bring out a radical fault in the analogy and something of the peculiarities of the logical behavior of normative expressions.

14. We know that the apparent color of an object varies with certain conditions, such as the source of light which falls on it, the physiological state of the observer, and so on. Let us call each set of mutually exclusive conditions which affect its apparent color a set of "differential conditions" of its apparent color. Now it is possible to analyze "the true color of an object *O*" as meaning "the apparent color of *O* under standard conditions," where by "standard conditions" we mean some specified set of conditions including one and only one member from each set of differential conditions. As a rule we tacitly adopt as standard conditions the conditions under which the object in

question is normally viewed; and of course some objects are normally viewed under different conditions than others: stage make-up and ships, for instance. In many cases our ordinary usage has not prescribed which conditions are to be taken as standard, and when we meet such cases we are free to decide the matter according to our convenience. In the case of stage make-up, for instance, it makes no difference whether we say, "His face is really such-and-such a color, but it will appear so-and-so on the stage," or whether we say, "His face looks to be such-and-such a color now, but really (on the stage) it is so-and-so." Perhaps the only condition we *do* normally cling to as standard is the normalcy of the physiological apparatus of the observer, but even this is merely a matter of (rather considerable) convenience. The crux of the matter is this: all the purposes of communication (except that of reasonable brevity) could be served without the notion of "true" color at all. We need not decide, for instance, which lighting conditions are to be taken as standard for stage make-up: we can say simply, "This face, as made up, will appear such-and-such in the dressing room, and so-and-so on the stage." We could do precisely the same thing in speaking of any other colored object (insofar as we knew how its apparent color varied with varying conditions); and if we then wanted the convenience of a set of standard conditions, we could make our choice (which is genrally made on the basis of convenience), on any agreed basis at all, without prejudging any substantial question.[7]

15. Thus if someone insisted that our list of standard conditions for determination of color were incorrect, we might want to argue the question; but we *could* very well say, "All right, let's use your set of standard conditions—as long as we agree what set we are using, it won't matter." But could we do the same sort of thing in the case of a word like "ought"? The "same sort of thing" would consist of saying, "All right, you are defining 'ideal observer' by a different set of criteria from mine; let's not argue over definitions: I agree to use your definition, so that the argument can proceed." What help could this be? Perhaps on our opponent's definition of "ideal observer" it would

[7] I do not mean to deny that there would be good reasons for choosing certain conditions as standard rather than certain others: it would not do, for instance, to choose a set of standard conditions such that it would not permit as many color discriminations as we could make under the conditions under which we should normally view colored objects. One could no doubt think of numerous other subtleties equally irrelevant to my present argument.

turn out that we "ought" to (say) foreswear euthanasia under any conceivable circumstances. But if we were genuinely in doubt about euthanasia, this could hardly be any help to us, because we would have to know, then, whether his definition of "ideal observer" and thus of "ought" was correct.

16. It is well enough to say, "If you take white light as one of your standard conditions, the ghost's face is greenish, but if you take the conditions on stage in the last act as standard, his face is a kind of bluish-black." This is answer enough: it would be pointless to insist, "But what is it *really*?" But the colonel could not say to the captain, "On one definition of 'order' I order you to arrest the lieutenant, but on another definition of 'order,' I don't." Here it is not at all pointless for the captain to insist, "Well, do you order me to, sir, or don't you?" What I want to say, of course, is that the case of "ought" is more like the case of "order" than like the case of "greenish." It is no good to us to know merely that if one set of characteristics are taken as those of the ideal observer, then we ought to do a given thing, while if another set are taken, we ought not: we must know what to do—the crucial thing about "I ought," like the crucial thing about "I order you," lies in its relationship to action.

17. Of course, there are cases where we cannot just say, "If you take one set of conditions as standard, his face is greenish, and if you take another it's bluish-black." Among the cases where we cannot do that are those in which we have been ordered or instructed or advised to *do* something which depends on the color of that face. For instance, if I am the make-up man, and the director tells me that he wants the face to be green, I must find out which conditions he is taking as standard when he gives me those instructions. Where this element of command (or advice, or instruction) is present, we must decide what to do, and we can no longer say merely, "If you take one set of conditions . . ., but if you take the other set. . . ." The point which I am suggesting, without arguing it in detail, is that some such element of practical guidance—of something like advice, command, instruction—is generally involved in the case of ethical expressions. (I should want to say, in fact, that this element—which I have not tried to describe fully—is one of the *essential* features of the use of ethical expressions; and I should want to take this kind of *use* as more important than the particular words so used, in deciding whether to count an utterance as an ethical judgment or not.) That is why there is in general such a difference between (a) the choice of standard conditions for the definition of "ought" and (b) the choice of standard conditions for the

definition of "green"; and that is also why the difference tends to vanish when the choice described in (b) is made in a context of command, advice, and so on.[8]

18. The reader will be aware that the intimate connection between ethical predicates and action has been noticed by such different philosophers as Kant and A. J. Ayer, so he will not jump to the conclusion that I adopt either Ayer's position or Kant's, or any specific one of those lying between (or around). I have not attempted to set forth my alternative to Firth's position. Even if my criticisms of him are correct, however, I feel sure that much of what he has to say would have an important role in a more adequate analysis of meaning in ethics than I think his paper achieves. It seems to me, for instance, that Firth's "ideal observer" makes a fine sketch of a moral *judge*, that is, one who applies rules to cases. This might be true even if I am right that the ideal observer, as defined, might not give a hang about moral codes themselves. For instance, our approval or disapproval of certain kinds of pacifism on the part of members of nonpacifist societies might depend on features of our psychology which could not apply to Firth's "ideal observer," so that he might have no interest in pacifism either way; but it might still be true that, given some moral rule relevant to pacifism, he would be a fine judge of its application to particular cases or kinds of conduct in specified situations.

19. In conclusion, perhaps I should very briefly relate what I have said to several other things which have been written on this subject. I think that Professor G. E. Moore's famous attack on the naturalistic fallacy, in *Principia Ethica*, did not succeed in bringing out why it is a mistake; and I look upon the foregoing as a way of showing that the "naturalistic fallacy" is at least a mistake, and that it is perhaps not too strong to call it a fallacy. If my argument is sound, Firth's analysis offers a very subtle example of a generic mistake about the "logic" of normative expressions (or better, of the normative use of expressions). R. M. Hare's treatment of "naturalistic" theories in *The Language of Morals* seems to me to be excellent, and my excuse for writing more on the subject is just that Firth's proposal is more subtle and ingenious than the sort which is discussed in Hare's book. I think that my arguments tell not at all against an attempt to state in non-normative terms the criteria of a good judge of moral issues, or to set up a proce-

[8] I do not mean merely that there is a difference between (a) and (b), of course; the point of this article is that the difference is so great that ethical terms *cannot* be defined by such a procedure.

dure for deciding moral questions, unless such an attempt is conceived as an attempt to characterize fully the *meaning* of ethical expressions.[9]

RICHARD G. HENSON

Cornell University

[9] Thus I think my argument has no bearing on Professor John Rawls's position in "Outline of a Decision Procedure for Ethics" (*Philosophical Review*, LX [April 1951]); but I think it is incompatible with certain aspects of the argument of Professor William Kneale on "Objectivity in Morals" (*Philosophy*, XXV [1950], reprinted in W. Sellars and J. Hospers, *Readings in Ethical Theory* [New York, 1949]).

CONCERNING THE LANGUAGE OF RELIGION

PROFESSOR WILLIAM KENNICK, in Section IV of his interesting article, "The Language of Religion"[1]—of which the first two sections seem to me beautifully clear and sound—formulates certain criticisms of a conception of religion which he mistakenly believes to be mine. What he does there is as follows.

(a) He ignores the definition of religion I give at the bottom of page 130 and top of page 131 (also, more fully, on page 415) of my book, *A Philosophical Scrutiny of Religion*, as "any set of beliefs that are matters of faith—together with the observances, attitudes, injunctions, and feelings tied up with the beliefs—which, insofar as dominant in a person, tend to perform for him privately, and for society through him, the respective functions I shall describe," one being personal and the other social.

(b) He quotes, correctly, the account of the *social* function of religion I give on page 135.

(c) He ignores (in the paragraph of his article immediately following that quotation from my book) the distinction, to which I devote two separate sections of my Chapter VIII, between the personal *rewards* of religious *conduct* (pp. 140-142), which on page 142 are specifically described as "contingent rewards for such conduct"; and the personal *function* of religious *beliefs*—which function alone is the function, performed *for the individual privately*, referred to in the definition on pages 130-131, i.e., made by me part of the *essence* of religion, as distinguished from a contingent reward of religiously motivated conduct.

(d) As a result of his ignoring the above, Professor Kennick states on page 69 of his article that, according to me, "the business of religion . . . is with altruistic attitudes and altruistic conduct *alone*" (italics mine). The "alone" is patently erroneous since, in my definition on pages 130-131 I refer to and include, and on pages 142-147 I describe in detail, the personal function of religious belief; and I do *not* present performance of it by religious belief as either a corollary or a contingent reward of altruistic attitudes or conduct, but allude to it as an "automatic psychological effect of the beliefs themselves" (p. 142), stating that to generate that kind of psychological effect (to wit, "a blessed state of mind of a unique kind, which enables him to maintain a degree of detachment and equanimity through both

[1] *Philosophical Review*, LXV (January 1956).

misfortune and good fortune") is the distinctive personal (vs. social) function of religion.

Professor Kennick's criticisms are thus criticisms not of my conception of religion, but of a conception which, as his word "alone" makes patent, is different from mine. His criticisms would be relevant to Kidd's and Marshall's conceptions, which (on pp. 121, 125, 130) I criticize because *they* ignore the personal function and define religion in terms of its social function *alone*, as Kennick erroneously asserts that I do.

As regards Satanism, I could say, with Kennick, that it is "a religious phenomenon"; but I would then add that there are good religions and evil ones; that the functions, social and personal, which distinguish the evil ones are those I mention at the top of page 196; and that a comprehensive account of religion has to consider both good and evil forms of religion, just as a comprehensive account of ethics has to consider both moral and immoral modes of conduct. But it seems to me equally legitimate to speak, as I do in my book, of "Religion" and of "Satany," and to say that they are related indeed—the relation being that each is the "inverse" of the other in the sense I state there.

The choice between these two accounts of Religion and of Satany is thus purely a terminological one, which does not make either acount a more or a less "persuasive definition" of religion than the other.

Finally, I submit that the kind of psychological state—call it an attitude or not—which I describe at length on pages 144 and 145, is capable of being as "pervasive" as any; and yet it is not religious insofar as pervasive, but because it is the particular sort of state or attitude there described; and it is religious even when it is not pervasive but is sporadic, periodic, or partial.

C. J. Ducasse

Brown University

BOOK REVIEWS

PLATO'S PHAEDO. A translation with Introduction, Notes and Appendices. By R. S. Bluck. London, Routledge & Kegan Paul Ltd., 1955. Pp. x, 208. 21s.

Dr. Bluck's book is published in the same series as F. M. Cornford's well-known translations and running commentaries, and in general it follows the same plan. The running commentary, however, is kept brief, and detailed discussion involving a knowledge of Greek is consigned to a number of appendixes. The translation is prefaced by an Introduction designed as well for those without as for those with Greek, in which the Theory of Forms in the *Phaedo* and the arguments for immortality, including Plato's later proofs of immortality, are discussed at some length.

First then the translation. As Bluck rightly says in his preface, "Although [the *Phaedo*] is, indeed, a masterpiece of dramatic literature, its author intended it to be first and foremost a work of philosophy." He has, too, readers in mind who may be unable to refer to the Greek text. It is important, therefore, that his translation should bring the reader close to the philosophical implications of the original Greek. This Bluck succeeds in doing without sacrificing too much of the dramatic power of Plato's writing. One example may serve. It is from the autobiographical passage at 99e, where Socrates is describing his philosophical career, and it is interesting to set Bluck's translation beside the original (third edition) Jowett translation and the revised (fourth edition) Jowett. The original Jowett runs:

> And I thought that I had better have recourse to the world of mind and seek there the truth of existence. I dare say that the simile is not perfect—for I am very far from admitting that he who contemplates existences through the medium of thought, sees them only "through a glass darkly," any more than he who considers them in action and operation.

The revised Jowett version is:

> And I thought I had better retreat to the domain of reasoning and seek there the truth of existence. I dare say that the simile is not perfect—for I do not quite agree that he who contemplates things through the medium of thought, sees them only "through a glass darkly," more so than he who considers them in their material existence.

Finally, here is Bluck's rendering:

> I decided that I should take refuge with *definitions*, and study the truth of

things by means of them. (Perhaps my analogy is in a way not quite right: for I don't altogether admit that he who studies things by means of definitions is using images more than he who studies things by means of physical examples.)

The original Jowett translation of the dialogues is, as the editors of the revised Jowett rightly say, "in its total effect a true work of art"; but the artistry at times obscures the philosophy of the original. The revised Jowett, as can be seen from the sample just quoted, seeks to remove some of this obscurity. For the study of the philosophy of the *Phaedo*, however, Bluck's translation is more useful than either, as I think the illustration shows. Even here one might want to quarrel with the translation of "*logoi*" as "definitions." Still, this translation does signal philosophical puzzles much more than either "the world of mind" or "the domain of reasoning," and Bluck is able in his commentary to bring out the philosophical difficulties in the Greek word "*logoi*," and his reasons for translating it here as "definitions." At 101a, however, he himself obscures the recurrence of the word "*logos*," which is perhaps not without some philosophic point there too, by translating "*enantios logos*" as "an opponent," when it might have been better to preserve the *logos* element and translate as something like "contradiction" or "contradictory statement." At 102a ff., because of the view he holds that a distinction has to be drawn in the *Phaedo* between Form-copies and Forms, he uses initial capitals for Forms, but not for what may be treated as Form-copies. It might have been preferable to avoid this device, giving in the first instance a straight, noncommittal translation of the Greek, without the use of capitals; though again the commentary gives a full explanation of what is afoot. In general, the translation can be strongly recommended as a reliable rendering of the philosophical ideas embodied in the Greek text.

There are nine appendixes, some dealing with points of detail in the text, some, like the sixth ("Platonic Forms: Are They Universals") or the eighth ("Platonic Forms: Why Were They Separated"), of more general interest. I shall confine my remarks to the longest of the appendixes, the sixth, on "Hypotheses and Logoi," which deals with the difficult passage at 99d ff. It provides a good example of the sort of discussion one may expect to find in the book, and at the same time, as Bluck indicates in the Preface, it offers a new theory about this passage which is one of the main innovations in his interpretation of the *Phaedo*. In general, then, Bluck thinks it a mistake "to read into the dialogue too much logic" (Preface, p. viii), and in the present appendix he denies that the 99d ff. passage is concerned with proposi-

tional reasoning. Lines 99d-100a are, he argues, concerned with Socratic definitions ("*logoi*" here are "definitions"), and the conformity of things with these definitions. At 100b ff., which purports to clarify Socrates' view at 99d-100a, Plato in fact "elaborates or 'clarifies' Socrates' view by developing it into his own theory of Form-causes (the causes concerned being separately existing Forms)" (p. 168), and "to show that we are now concerned with an advance upon Socraticism, the term hypothesis replaces the earlier Socratic logos" (p. 166). But here again "hypotheses" are not primarily propositions, and again we are not concerned with propositional reasoning. "Hypotheses" are provisional "notions" (this is Bluck's word) of Form-causes, and "the comparison of the 'results' of a 'hypothesis' is one way of testing the validity of one's notion of a Form-cause" (p. 162). It is not, that is, the making of deductions from one or more propositions that is involved here, but rather the seeing "whether all those things which, on the assumption of the correctness of our notion of the cause, would have to be accepted as all alike caused by that cause, can, in fact, be admitted to be so caused" (p. 169). In the *Republic* (509d ff.), Bluck understands the "hypotheses" there in a similar way—they are not propositions, but notions of the nature of Forms, "provisional mental images" (p. 163).

Bluck argues his case with a skill and scholarship to which this brief outline does scant justice. Nevertheless, he has not succeeded as yet in convincing me that Plato has not some kind of propositional reasoning in mind in this part of the *Phaedo*, or in persuading me that Ross (*Plato's Theory of Ideas*, ch. iii) or Robinson (*Plato's Earlier Dialectic*, ch. ix) are on the wrong tack. To indicate very briefly, and without developing them, some of the difficulties I find in Bluck's account: (1) The transition he sees at 100b from the Socratic theory of definitions (accepting for the moment Bluck's view that "*logoi*" at 99e means "definitions") to a quite different theory of Form-causes is peculiar. The transition, Bluck says, "is effected by the device of making Socrates offer to explain his meaning 'more clearly' " (p. 165). But it seems heartless on Plato's part (though I admit that in his case it is not inconceivable) that he should offer to explain one theory more clearly by producing an entirely different one. It looks as though there must be some real linkage between the earlier *logoi* and the later hypotheses. (2) Robinson, in *Plato's Earlier Dialectic*, Chapter VII, discusses in detail Plato's use of the verb "to hypothesize," maintaining that what is hypothesized is either a proposition or sometimes a proposal, and that the noun "hypothesis" takes its meaning entirely

from the verb. Thus the noun has, four times, the practical "proposal" sense of the verb, but in every other occurrence "it means a proposition posited as a start towards a system of propositions" (2d edition, pp. 98-99). Now Bluck can of course reply that Robinson has missed a special use in the *Phaedo* and the *Republic*; but a full discussion of the point seems to be required. As it is, he only refers (p. 162, footnote), without explanation, to the use of hypothesis "in the sense of 'notion' " in Xenophon, *Mem.* IV. vi. 13, where, it seems to me, the word in fact means "the question under discussion," i.e., a proposition of some kind or other, and not a "notion," nor, still less, a "provisional mental image." (3) Still in connection with his view of a hypothesis as a "notion," a "provisional conception of a Form-cause," Bluck frequently speaks of the "results" of such a "notion." But what exactly, then, does he mean by "notion"? As I have already indicated, when discussing the *Republic* he says of the hypotheses there that "they are not propositions, but provisional mental images." Elsewhere, however, (p. 169) he uses the word "supposition" in this connection, and in a footnote on page 170 he speaks of "our notion (hypothesis) that the Form of Fire includes the nature of 'the hot'," where in both cases "notion" seems to be "notion that . . .," and to be propositional in form. Certainly it is not clear how a "notion" in the sense of "mental image" could have any "results" in any sense of the word relevant to the present context, or "lead to" either consistencies or inconsistencies. Bluck, in speaking of the "consequences" or "results" of a hypothesis (taken in his sense of "notion of a Form-cause"), usually employs quotations marks. This, I take it, means that these words are being used here, not in a logical sense, but in some unusual sense; but the difficulty is to give a workable account of this unusual sense in the present context. (4) Similarly, it is not clear in what way Bluck is using the word "cause" when he talks of "Form-causes" in the *Phaedo*. It seems clear that he rejects Ross's view (*Plato's Theory of Ideas*, p. 29) that it is formal causes that are in question. Is he then using "cause" as "efficient cause"? But if so, Aristotle's criticisms of the *Phaedo* (e.g., *Met.* 991*b*) on this score are relevant, and so also is Mr. Murphy's nice remark (*Interpretation of Plato's Republic*, p. 147): "Socrates [in the *Phaedo*] is not a coroner investigating the causes of death." Occasionally here too Bluck resorts to quotation marks: "Forms are 'caused' by 'higher' Forms" (p. 170); "Forms (the 'causes' of these characters)" (p. 172); and again one wonders what the quotation marks signify. On pages 16-17 he explains that "Platonic . . . Forms may be regarded as *causes*" (italics, but no quotes) and then summarizes part of the

text of the *Phaedo*, remarking at the end "in that sense the Form is the 'cause' of its [a thing's] existence," where the quotation marks would seem to emphasise that we have to understand what is meant by "cause" from the text. But I suggest that there would still remain a puzzle as to what either Bluck, or Plato, if Bluck is correct in his interpretation of the *Phaedo*, mean by "cause" when they speak here of Forms as causes. To say this, incidentally, is not to malign Bluck; for the concept of cause as it occurs in Greek philosophy is, it seems to me, an extremely tricky business—compare, for example, the remarks on this topic from a different angle by M. B. Foster in Part I of his article "Christian Theology and Modern Science of Nature" in *Mind*, XLIV, n.s., no. 176.

These, then, are some of the reflections which lead me to remain unconvinced by this interesting thesis. I hope that in offering them I have at the same time given the prospective reader some idea of the sort of discussion and of the careful and scholarly work that he will find in this book. I warmly commend Bluck's work to all students of Plato's philosophy, as both a very useful translation of the dialogue and a stimulating and well-documented commentary.

R. C. Cross

King's College,
University of Aberdeen

MORAL JUDGEMENT. By D. Daiches Raphael. New York, The Macmillan Company, 1955. Pp. 224. $2.75.

In this book Professor Raphael moves away from his earlier rationalistic intuitionism (expressed in various places in *The Moral Sense*) toward a naturalism of the Ayer-Stevenson variety, but not without nostalgia. He is obviously still attracted by the conceptual cleanliness and articulateness of the English intuitionists—and why shouldn't he be?—but is disturbed by their metaphysical commitments. So he tries to retain the one while cutting loose from the other. If this effort is meant to resolve what has been called "the present deadlock in ethics," I doubt its success; it may be, however, that Raphael does not so consider it, but views it as a change of allegiance on his part and an offer to his new associates of a substantial gift of matériel from the camp of his old confederates.

There is a curious reflection of this ambivalence in the formal structure of the book. The table of contents presents an apparently

clear-cut division; it tells us that Part I is on the logic, Part II the metaphysic, of morals. This suggests that the first part is a syntactical study of ethical language, the second an exploration of epistemological and ontological commitments or assumptions. This characterization is accepted by the author (adding that psychological matters are also considered in the second part), and not without justification. But he also gives other descriptions that are difficult to square with this. For one thing, he depicts the first part as a discussion of moral concepts, the second as an investigation of the nature of moral judgment, whereas one would suppose that a syntactical and again a metaphysical study is possible on each of these topics. Moreover, he further represents the first as a search for a common criterion of right action, the second as a consideration of the naturalism-nonnaturalism issue in ethics. I would, I think, characterize the first as an attempt to simplify and co-ordinate ordinary English language about morals on a deontological model, the second as a somewhat hesitating proposal to give this simplified language a naturalistic interpretation.

When dealing with the moral terms found in everyday parlance, Raphael takes his stand unequivocally with the deontologists. He contends, rightly in my estimation, that the idea of moral obligation or duty is, in ordinary thought, quite irreducible to completely nonmoral notions or even to concepts of goodness or fittingness (plus nonmoral ideas):

> Analysis of obligation in terms of fittingness does not bring out the essential meaning, which is bindingness on the agent. . . . Again, the goodness or fittingness is not something that now exists but a characteristic that would exist if he did the act, while his obligation exists now before he acts; it is an obligation to act [p. 58].

But now for our author's naturalism (in Part II). As a first step in understanding it, we note his "ultimate principle of morals" which can serve to bind together all our particular obligations; it is that "we ought to treat other persons as ends in the way we naturally treat ourselves as ends" (p. 167). As a second, we observe that he offers this as "a postulate, a general policy of action, to which there could be alternatives" (*ibid.*); apparently this means that we are to accept or reject it according as it does or does not imply particular imperatives of action that we are willing to live by. So far, of course, we have not left nonnaturalism (though we have left a rationalism of necessary connections in this area). As a third act, then, we remark that this ultimate principle, although not reducible to, yet arises from, a sympathetic feeling for others' good. There is an imaginative trans-

action whereby one comes to regard oneself as related to another person's good as one is to one's own, namely, to be disposed to seek it. As a final measure, we perceive that the concept of goodness or fittingness comes from that of interest, that is, from a nonmoral, a "natural" character.

We thus have before us a genetic hypothesis:

> Like my account of fittingness or good, this account of moral obligation may be called "naturalistic," since the original object of reference giving rise to the idea of obligation is something that exists in the world of nature [p. 124].
>
> It will not do to present a naturalistic theory as a theory of the *meaning* of ethical terms [for that] is part of the logic of morals, and there the facts with which our theory must accord are what people making moral judgements have before their minds. A sensible naturalistic theory should be one like Hume's. He tries to build up . . . a theory which will *account for* the existence of moral concepts and judgements as they are ordinarily used. A theory of this kind is largely a genetic theory, not an analysis of meaning [p. 169].

Now this may seem straightforward enough: the "logic of morals" shows that ideas of moral obligation and fittingness cannot, once they have arisen, be reduced to nonmoral ideas of what is in another's or one's own interest; the "metaphysic of morals" presents a hypothesis concerning the psychological genesis of the former from the latter. Unluckily, we cannot rest with this, for our author frequently writes in such a manner as to make it almost imperative to treat his "naturalism" not as a genetic hypothesis but an ontological simplification and a semantical indentification:

> If a naturalistic theory can do the job it sets out to do, of explaining moral concepts . . . , it is to be preferred. The principal reason for seeking such an explanation is the desire for economy, exemplified in Occam's razor. This desire is due to the metaphysical aim of synthesis or unity; one world of entities, if possible, not two or several [p. 171].
>
> But I agree with the naturalists that the entity to which the word 'good' ultimately refers is interest [p. 119].

Moral words describe natural facts; they cannot be replaced by ordinary descriptive terms, but this is simply because they have an evocative or imperative force in addition.

Now, not only is Raphael encumbered with two accounts, if this analysis is correct, but on neither does he resolve the deadlock in ethics nor produce a successful synthesis; for the one is deontological and nonnaturalistic in ethics, bringing in naturalism as a psychological hypothesis only, the other is clearly naturalistic both semantically and ontologically.

I am perhaps to be reprimanded for using up my space on what is to

me (and I take it to Raphael) the central issue in the book. For I must neglect his interesting and frequently insightful excursions into such more peripheral topics as justice and its relations to desert, liberty, and equality; the classification of obligations; the problem of free will and the meaning of the term "cause."

EVERETT W. HALL

University of North Carolina

AESTHETICS AND CRITICISM. By HAROLD OSBORNE. New York, Philosophical Library, 1955. Pp. vii, 341. $6.00.

Many critics and philosophers share the traditional view that a theory of art is basic to any adequate critical practice upon it. This view is part of the total doctrine that any particular claim about the world implies or presupposes a theory of it. In the philosophy of criticism, it has traditionally been maintained that unless a true definition of the necessary and sufficient properties of art (a theory of it) is forthcoming, criticism is a meandering and worthless enterprise. Without a sure knowledge of what art really is, nothing critics say about it makes sense because their language lacks a clear referential dimension. Critical talk about art has no meaning unless the property that makes art what it is is specified, since it is that property which is the referent of its talk.

In the book under review, Mr. Osborne accepts completely this traditional connection between theory and meaning. In their utterances and arguments about art, critics, he contends, inevitably make evaluations where the aesthetic predicates must be related to their meanings, which are the defining properties of art. Any critical judgment, such as "*X* is good" or "*X* is better than *Y*," can be true only if "good" or "better than" names a defining property. To say of a work of art, then, that it is good, is to imply that it possesses *P*, where *P* is the defining character of artistic excellence. Thus, if a critic says of a work of art, "This is good because it is profound," Osborne argues that such an utterance implies the aesthetic theory that art is essentially profundity; and hence that it is profundity that distinguishes art from everything else, and that artistic excellence is identical with profundity. Most of his book, therefore, is an attempt to show that all classical definitions of *P*, with the exception of his own, are false because they do not function as statements of the necessary and sufficient properties of art.

There are many serious difficulties in Osborne's general approach. In the first place, it is extremely doubtful that a theory or definition of art could ever be forthcoming, since the concept of art itself is simply not amenable to definition or theory. Secondly, his identification of meaning with reference, itself a mistake, makes it impossible for him, from the very beginning, to understand critical talk. For him, all talk must be about something in the same way, in order to be meaningful, so that the terms of criticism derive their meanings from what they denote. There is no inkling, in his book, of the possibility of interpreting critical judgments as basically nonreferential language. There is no reason whatever why "This is [aesthetically] good" should function like "This is bulky"; and yet his whole program reduces to an assimilation of aesthetic and evaluative talk to descriptive talk.

Osborne's worst initial mistake, however, is to confuse the procedure of giving reasons for critical evaluations with implied definitions of aesthetic excellence. Surely the critic who says "*X* is good because it is profound" cannot be said to imply a whole theory of art as profundity. Indeed, he may have no theory of art at all. He may not even wish to generalize from his utterance to "All profound works of art are good," let alone define art as profundity. To say "*X* is good" is to imply some reason for saying it, but the reason may not be an aesthetic theory. In fact, if no reasons could be given which were not based on theory, then if there were no theory of art forthcoming—which there is not—criticism would be impossible. This is manifestly absurd.

In Chapters III-X, Osborne examines the great classical and contemporary theories of art which have served as the basis of criticism. In each case, with the exception of his own organic theory, he argues that it defines art in terms of some property which is neither necessary nor sufficient or which is dubious or nonexistent. Hence, the critical judgments which are based upon them are false, for they imply, when they are of the form "*X* is good," that goodness is identical with a property which does not define art.

The first of these theories he calls "Illusionism," according to which art is a special kind of making—of imitations or reproductions of something else. Plato and Aristotle championed this theory, mixing it with a kind of moralism whereby artistic excellence is identified with imitations that result in good citizenship. This notion that artistic excellence consists in realistic fidelity dominates medieval and Renaissance aesthetics as well. In painting, Osborne points out, Courbet and French Impressionism carried illusionism to its breaking point. Post-Impressionism, building on the previous emphasis on reproducing

exactly what is seen, led to the defeat of illusionism by showing that sense perception itself is selective. Expressionism was the natural result.

Nevertheless, Osborne goes on, realism has hung on, and its basic question, "In what sense can a work of art be, and to what extent should it be, an exact replica of something else?" remains in contemporary aesthetics. The whole notion of art as symbol is a variant of illusionism. Osborne discusses Langer's view that art is "presentational symbol," commenting that literature and visual art could be construed as mimetic but that music could not be so regarded.

Next, Osborne examines the theory that art is essentially truth. Art is not a collection of deep insights into reality. Many works of art are unconcerned with truth, and many true tracts are not art. Further, on this view, architecture and music, which are incapable of making assertions, would be ruled out as art.

Expressionism fares no better. This is the theory that artistic excellence consists in the power of a created artifact to communicate experience and to express the artist's personality. In its sophisticated form, it holds that art acts as a symbol of the artist's mind in that it serves him as a medium of self-expression and causes us to know that state of mind. Osborne rejects this view on the grounds that it rests on a notion of artistic intention which is strictly speaking unverifiable so far as the critic is concerned.

Transcendentalism, the theory that art is a revelation of higher truths, a kind of emotional, nonconceptual knowledge, Osborne also rejects as being unempirical and hence quite unserviceable for critical theory.

There is one and only one true theory of artistic excellence, Osborne concludes, namely, the organic one. According to this view, artistic excellence has nothing to do with representation, expression, evocation, or truth. It is essentially configuration. Each work of art is an informing of materials into a formal structure, a configuration, such that the whole is prior in our awareness to the parts and their relations. Following C. D. Broad on organic unity, Osborne contends that a work of art is a whole or organic unity such that no part of it could have existed unless all the other parts had existed and had stood to each other in the relations in which they in fact did stand. Unlike mere visual fields, in which there is causal influence among the parts, the work of art is that kind of whole that can enter into awareness in a single act. Osborne calls this feature "compactness."

The critic, then, is one who has skilled himself in total apprehension

of artistic wholes and the ability to discriminate among the parts and relations without losing this awareness. On the basis of this, the critic records and compares the results of his awarenesses of works of art and describes these wholes in order to get others to enter into similar awarenesses. Criticism, thus, Osborne concludes in his final chapter, has two functions: to exhibit a true order of merit among works of art in respect to those qualities of excellence they possess as works of art; and to describe works by calling attention only to those characteristics in virtue of which they are judged to be excellent or indifferent.

In my opinion, Osborne's theory is no better than those he rejects. Even if one could make sense of the notion of organic unity, the way in which he employs the concept does not help. For, on his view, organic unity becomes a property of art along with the other properties it has; and it is always possible to ask whether that work which has this property of organic unity is good. Osborne can have no answer to this, and no one can until he abandons the search for evaluative properties in art that supposedly correspond to our critical evaluative terms and asks instead: What sort of roles do critical terms play in the actual writings of critics? The basic difficulty with Osborne's approach is that he begins by assimilating these roles to the one of referring to properties.

MORRIS WEITZ

The Ohio State University

LANGUAGE IN CULTURE. Proceedings of a Conference on the Interrelations of Language and Other Aspects of Culture. Edited by HARRY HOIJER. Chicago, University of Chicago Press, 1954. Pp. xi, 286. $4.50.

The views of the late Benjamin Whorf on the interrelations between language, culture, and mental processes continue to evoke keen interest. There is certainly something attractive about the idea that a language embodies the *Weltanschauung* of an entire culture. Anthropologists welcome the contention that basic patterns of interpretation of the world vary from one culture to another and are willing to believe that language may have a specific and variable influence on perception and thought; while philosophers can hardly be indifferent to Whorf's claim that "supposed necessities of thought" are mere reflections of linguistic structure. But Whorf's views are elusive: he was not an exact thinker, and some of his best-known articles were written for a popular audience. One turns eagerly, therefore, to this report of a

conference in which a group of tough-minded specialists—mainly anthropologists and linguists, with a couple of philosophers for good measure—recently examined (by the grace of the Ford Foundation) some of the questions arising from "the Sapir-Whorf hypothesis." The seven papers here reproduced were prepared in advance of the conference; the volume contains also some 150 pages based upon the discussions that ensued.

The best introduction to the argument is probably the paper by Harry Hoijer, which provides a useful summary of Whorf's views (though somewhat less fully than in the same author's "The Relation of Language to Culture" in *Anthropology Today*, by A. L. Kroeber and others [Chicago, 1953], pp. 554-573). Hoijer's target is the view that "language functions, not simply as a device for reporting experience but also, more significantly, as a way of defining experience for its speakers" (p. 93). Hoijer's own studies of Navaho culture lead him to support some version of this "central idea." A certain "dominant motif belonging to the Navaho thought world" (p. 101) harmonizes with a characteristic "fashion of speaking" in the Navaho language. The Navaho believes that Nature is more powerful than man, so that the good life calls for discovery of the proper and relatively passive relationship between man and the superior forces around him. "Navaho man lives in a universe of eternal and unchanging forces with which he attempts to maintain an equilibrium, a kind of balancing of powers" (p. 100). This attitude in turn is said to be reflected in certain structure of Navaho verbs, in which the speaker links

> individuals to actions and movements distinguished, not only as actions and movements, but as well in terms of the entities in action or movement. This division of nature into classes of entity in action or movement is the universe that is given; the behavior of human beings or of any being individuated from the mass is customarily reported by assignment to one or other of the given divisions [p. 102].

I give this somewhat lengthy quotation in fairness to Hoijer, though I confess I do not fully understand it. However, it is certainly plausible that a broad view of the universe as unchangeable and demanding harmonious conformity might *somehow* be reflected in the language. Whether any particular features that may be isolated are significant must be left to the experts to decide.

Hoijer is not an uncritical devotee of Whorfianism: he makes the good point, against hasty inferences from grammar or vocabulary, that the linguistic patterns relevant are the "frequently used, and relatively simple structural-semantic devices; those, in short, which

are common to the adult speech community as a whole, and are used by its members with the greatest ease" (p. 97). He deals scrupulously with the objection that related cultures may have languages of very different structures, discusses problems of translation (not insoluble, in his view), and lays down good proposals for further research. (Readers interested in pursuing these questions further might turn to Hoijer's paper, "Cultural Implications of Some Navaho Linguistic Categories," *Language*, XXVII [1951], 111-120, for some reason omitted from the bibliographies of the book under review. It is one of the best available applications of Whorf's ideas to a specific language.)

Joseph H. Greenberg's "Concerning Inferences from Linguistic to Nonlinguistic Data" is a careful and enlightening analysis of the various types of correlation between linguistic and extralinguistic data that may profitably be sought. He pays especial attention to inferences that may be drawn from variation (as between different languages) of the stock of what he calls "elementary meaning units," a term that covers the meanings of elementary grammatical constructions as well as the meanings of items of vocabulary and holophrastic idioms. The best we can hope for, he believes, is to find significant *statistical correlations* between such units of meaning and selected features of cultural behavior: "The attempt to establish priority of the verbal or nonverbal aspect seems to resemble the proverbial situation of the chicken and the egg" (p. 9). And an assumption of the existence of such statistical correlation "is simply a rule for forming hypotheses" (*ibid.*). An assumption that the "compulsory categories" of a language determine "conceptualization" can easily degenerate into "a mere tautologic restatement of the linguistic fact" (p. 12); inferences about culture drawn from any given feature of a language are highly vulnerable and need independent nonlinguistic corroboration. One final point impresses me as particularly worth making, viz., the difficulty of deducing from the *structure* of language the sophisticated metaphysical *propositions* that Whorf claimed to detect. Greenberg, in fact, takes a somewhat cool view of the possibilities of the "Sapir-Whorf hypothesis," though he does not deny the value of the search for correlations between partial linguistic systems and limited aspects of relevant nonlinguistic behavior.

Franklin Fearing's "An Examination of the Conceptions of Benjamin Whorf in the Light of Theories of Perception and Cognition" is the longest paper in this collection. Much of it is a review of such psychological and anthropological literature as bears upon the possible relativity of perception in different cultures. His most striking conclusion is:

> The evidence seems conclusive that there is a type of cognition in which the individual is in immediate contact with the external world, that in its simplest form it is probably the same for all men irrespective of culture, and that the symbolic processes, including language, are not necessary for it to occur, although when linguistic patterns are available they may be utilized to express it, probably in the form of metaphor [p. 72].

This supposed primitive type of response to stimuli is here characterized as "syncretic" and "physiognomic"; it is said to be marked by "the blurring or abolition of the subject-object polarity" (*ibid.*) and fusion of the cognitive, affective, and conative aspects of perception. Some of the evidence cited sounds more like conjecture than well-established fact; one wonders whether all psychologists would regard the evidence as "conclusive." The chief application to Whorf's view is that, if Fearing is right, Whorf neglected the existence of "ways of cognizing the world which exist not only prior to but concurrent with those involving concepts and symbols" (p. 78). I am not clear that this charge has much gravity. Again, it is no doubt true that Whorf had nothing to say about "the dynamic properties of the communication situation" (*ibid.*); but then where is one to find *anything* illuminating on that subject?

Stanley Newman's brief paper, "Semantical Problems in Grammatical Systems and Lexemes: A Search for Method" contains some interesting examples from the Yokuts and Zuñi languages of the technical problems that arise in determining "the dominant conceptual categories of a language" (p. 82). Charles F. Hockett's "Chinese Versus English: An Exploration of the Whorfian Thesis," discusses a number of specific differences between the two languages that might conceivably have a bearing upon the relations between language and culture. His main conclusions (presented tentatively as probably holding for all languages) are: "1. The most precisely definable differences between languages are also the most trivial from the Whorfian point of view. . . . 2. Languages differ not so much as to what *can* be said in them, but rather as to what it is *relatively easy* to say. . . . 3. The impact of inherited linguistic pattern on activities is, in general, least important in the most practical contexts, and most important in such goings-on as story-telling, religion, and philosophy—which consist largely or exclusively of talking anyway" (pp. 122-123). All of which is sensible and judicious, but does not take us much further.

The remaining two papers have rather less direct bearing upon Whorf's views. Norman A. McQuown's "Analysis of the Cultural Content of Language Materials" outlines some features of the research methods to be employed in comparing culture and language (see

especially pp. 26-27). The hope is expressed that this program will "make possible in the future controlled investigation of all the patterns of culture" (p. 31). "Shawnee Laws: Perceptual Statements for the Language and for the Content," by C. F. Voegelin, John F. Yegerlehner, and Florence M. Robinett, is a detailed analysis of certain Shawnee texts. These are said to exemplify two "perceptual categories" labeled "Eventful" and "Contemplative" respectively. No doubt this kind of investigation repays the enormous amount of labor that it obviously involves.

The "Discussions" on these papers present a fascinating picture of men trained in precise investigation attempting to come to grips with the intangible. They try hard to bring Whorf down to earth—or to the campfire, as it were. But as one difficulty after another emerges, the thesis of "linguistic relativity" seems to become progressively less plausible. At one point, Abraham Kaplan (one of the two philosophers participating) makes an ingenious attempt to show how a language might conceivably dispose its users to a variant of Platonism (pp. 139-140. Cf. also his fable of the different shapes of shovel as affecting the kinds of holes that result, pp. 173-174). But the field worker is quick to point out that in practice "it does not come that way" (Kroeber, p. 140). At one point, the whole discussion almost bogs down over the question whether the supposed correlation of linguistic structure and "conceptualization" can be more than a concealed tautology (pp. 221-225). Some of the professional linguists present are clearly uncomfortable about suggestions that questions of meaning can have a direct bearing upon linguistic analysis. But on the whole, the members of the conference seem to have agreed with Hockett's dictum that "Ethnography without linguistics is blind. Linguistics without ethnography is sterile" (p. 225)—and about very little else. One is left feeling that a good deal of further clarification of the views advanced by Whorf is desirable, if any good research is to result. Until there is more agreement about *what* the thesis is, verification is apt to be futile.

As usual in such "interdisciplinary discussions," one picks up fascinating scraps of miscellaneous information in passing. For those who do not despise the "anecdotal approach," I will draw attention to the Bororo of Brazil, who are "said to be incapable of noting the features common to all parrots" (p. 8) and have only two distinct number words (equivalent to "one" and "two"), but can count at least up to thirty-eight (p. 129). One may also learn here about the difficulties of speaking democratically in Swedish (p. 256); the old

Chinese doctrine of "the will of heaven," which justified either obedience or revolt at need (p. 272); the Navaho color system, which lumps blue and green together, but subdivides "black" (p. 96); the peculiarly impersonal way in which Totonac women must refer to their husbands (p. 159); and an analysis of the rules for bidding at bridge, viewed as a model "language" (pp. 146-147). These delightful illustrations add much to one's enjoyment of these stimulating but inconclusive discussions.

MAX BLACK

Cornell University

THE PRAGMATIC HUMANISM OF F. C. S. SCHILLER. By REUBEN ABEL. New York, King's Crown Press, 1955. Pp. xii, 207. $3.50.

The pragmatic movement in philosophy is usually associated with the Americans Peirce, James, and Dewey, and their American followers. The Oxford pragmatist F. C. S. Schiller is sometimes mentioned, but his "humanistic" brand of pragmatism has been all but forgotten in contemporary discussions of the movement. This small book provides a brief summary of Schiller's entire philosophy, along with a critical appraisal of it. The book will be particularly useful to those who want a fair and balanced understanding of all the important versions of pragmatism, and especially of the pragmatic conceptions of logic and knowledge. More than half of Mr. Abel's text is devoted to an examination of Schiller's theory of logic, his epistemology, and his philosophy of science, and this, the best part of the book, is an important contribution to the literature on the pragmatic viewpoint in these areas.

Some of the central ideas in Schiller's pragmatism, no longer associated with his name, are now drawing the close attention of many British and American philosophers. For example, Schiller contended strongly against men like Bradley and Bosanquet that the meaning of words and sentences was to be found in their actual use, in their function within the processes of knowing and communication. He also maintained that the principle of determinism was not a law of nature or an ontological principle but a postulate by which "we deny that any question is in theory unanswerable." Schiller's conception of the logical status of laws of nature is also interesting. In 1912 he wrote: "The impossibility of 'breaking' a Law of Nature proves nothing but our determination to uphold a phraseology we have found convenient.

It is by our convenience also that we determine *which* alternative to prefer, when a law is confronted with an apparent exception."

Schiller's theory of logic developed out of his attack on formal or traditional logic as incapable of application to actual thinking and knowing. Neither Schiller's own analysis of formal logic nor Abel's presentation of it answers satisfactorily the two major questions it raises: (1) For what purpose do we do formal logic? (2) What purpose is formal logic best fit to serve? In his zeal to deprecate traditional logic and the "intellectualism" of the absolute idealists, Schiller displayed serious philosophical weaknesses which Abel, a sympathetic critic, is honest enough to admit. He points out that Schiller failed to appreciate the distinction between analytic and synthetic statements. He shows that Schiller did not carefully distinguish "a study of the process of thinking (psychology) and a study of the validity of the resulting thought (logic)" and that Schiller therefore did not understand how the two are related. Abel describes Schiller's attitude toward symbolic logic as an "Olympian scorn," which he admits was "unfortunate" since Schiller really agreed with much that symbolic logic has done. Schiller also felt antipathetic toward the logical positivists, even though his type of pragmatism implied the verifiability criterion of meaning, and he explicitly condemned "the philosophic fondness for systems which no exterior fact could upset." Other critical points well defended by Abel are that Schiller did not understand the nature of probability; that he believed the laws of identity, contradiction, and excluded middle were "postulates" of the same sort as that of the conservation of energy; that he did not make clear what he meant by the concepts of usefulness and working, by means of which he defined "truth"; that his "personalistic" view of meaning could not do justice to the fact of the communication of meaning; and that he did not always avoid the genetic fallacy.

Notwithstanding these criticisms, the author still makes a good case for the claim that most of Schiller's writing on logic and scientific method warrants further study by contemporaries.

Abel's treatment of Schiller's metaphysics and theory of value is very sketchy and at times quite careless. For example, he makes little effort to bring together two very different conceptions of metaphysics, both attributed to Schiller: that metaphysics is an "ultimate synthesis" of the particular sciences, not different in kind from scientific knowledge, and that, in Schiller's words, "every . . . metaphysic is a poem." In discussing Schiller's idea of the evolution of the laws of nature, Abel, following an increasing number of popular interpreters of

science, commits the blunder of comparing the "beginning" of the expanding of the universe, now set at about five billion years ago, with Genesis. This is simply to overlook the radical difference in the *logic* of the two expressions: "In the beginning God created the universe" and "The expanding universe began its evolution five billion years ago." And finally, it is surely an unfair disservice to Schiller and to all pragmatists to liken any of their religious concepts to the extreme anti-intellectualism and deliberate irrationality of Unamuno's dictum: "To believe in God is to desire His existence, and, what is more, to act as though He existed."

PAUL W. TAYLOR

Brooklyn College

ROYCE ON THE HUMAN SELF. By J. HARRY COTTON. Cambridge, England; Cambridge University Press, 1954. Pp. xiv, 347. $5.00.

It is a curious phenomenon, though perhaps one to be expected in a young country like America, that the philosopher who came closest to producing a system of philosophy after the fashion of the classical philosophers should so soon be allowed to fall into virtual oblivion. It seems that what Professor Cotton has said of Royce's logic is true of the whole of his philosophy: "No one has developed Royce's system." Not only did Royce stand alone in his raising and grappling with perennial metaphysical issues, but he discussed problems in politics, ethics, and the philosophy of religion which are very much in the foreground at present. Moreover, he was a herald of what is now known as symbolic logic, and besides his own work in this field, his paper "The Problem of Truth in the Light of Recent Discussion," read at Heidelberg in 1908, still deserves attention as a brilliant defense of the importance of the new logic and especially of the sort of work then being done by Russell, Peano, Frege, and Schroeder. The neglect of Royce the idealist metaphysician is intelligible enough in view of present trends in philosophy, but neglect of Royce the logician and semiotician is less easy to fathom.

No less unfortunate than the disappearance of Royce's name from the philosophical scene is the fact that whereas studies of his thought have appeared in several European languages, Cotton's book is the first full-length study in English devoted to all features of Royce's thought. My own short work, *Royce's Social Infinite*, while it was devoted to Royce alone (previously the only writing in English about Royce had been in articles and in brief sections of books dealing with the

history of American philosophy), dealt with only a part of his thought, the theory of interpretation and community.

Cotton's book deals with all of the many topics upon which Royce wrote, from his earliest papers to *The Problem of Christianity* and related works on the community. In addition to printed articles and books, Cotton has made use of the great quantity of manuscript material which has survived. The author had his own doubts about the use of this material, as he candidly confesses (p. xi). I believe he has relied upon it too heavily, especially since so much of it is in fragmentary form and printed works themselves contain more careful statements of most of the points. On the other hand, it must be admitted that some unpublished lectures do throw light on Royce's interpretation of science and the working of thought in different scientific disciplines.

Cotton is on sound ground in his choice of the self as a topic central enough to serve as a vantage point from which to survey the whole. While it is untrue to the main drift of Royce's metaphysics to attempt, as some have done, to transform him into a "personalist," the fact remains that the nature of the self was a central problem for him, and if we consider the self in turn as knower, moral agent, religious believer, and above all, social animal, we shall find ourselves dealing with all of Royce's philosophy.

The author has adopted a chronological pattern which enables him to show the development of Royce's thought, and he has been very successful in combining both clear exposition and criticism. My only general complaint is that Cotton has been too conscientious in his effort to bring every utterance of Royce's into the discussion. Frequently he breaks off his own clear and incisive criticisms simply to give further quotations which, though generally apt, serve only to lay more stress upon a point. Royce, I am sure, would have advised the author to continue with his enlightening criticisms even if it meant that much had to be left out.

Two specific points may be singled out for further comment; the distinction between the internal and the external meaning of ideas and the relation between immediacy and mediation in the type of experience characteristic of the absolute knower. Cotton is correct in his contention that Royce's distinction between the internal and external meaning of ideas is more important than that between appreciation and description (see p. 84). In fact, the basic importance of the two types of meaning bears out the thesis of Dewey that the epistemological situation was the initial determinant of Royce's metaphysics. The argument in *The World and the Individual* offers further support in that

the question: What is Being? is taken to be the same as: What is Truth? and, in accepting part of the position he described as Critical Rationalism, Royce said, "To be real is to be the object of a true idea" (*World and Individual*, I, 339).

Cotton calls attention to a serious ambiguity in Royce's formulation of internal meaning (pp. 86 ff.) in that it means both all of our human purposes and interests, of which the cognitive is only one, and the purely logical or cognitive interest at the same time. Perhaps a better way to express the ambiguity, which is undoubtedly present, is to say that Royce referred to both the "purpose of an idea" and to the "purpose of a self," self denoting a whole whose limits fall beyond any single idea entertained at a single moment. There is, of course, a sense in which the purpose expressed in a given idea can be regarded as the purpose of the self intending it, but this does not alter the fact that when Royce spoke in the same paragraph about the purpose of the self, he meant not the single idea but a whole life plan or system of ideas and their relations. Cotton expresses the ambiguity as that between the cognitive interest and other interests because he wants to make the point (pp. 101 ff.) that Royce was blind, or nearly so, to the influence of interests other than the cognitive upon the enterprise of knowing. I believe that Cotton is right in his basic criticism in that man is not always engaged in seeking theoretical knowledge, nor is his entire life summed up in this pursuit, but in making this point Cotton is apt to miss the other side of Royce's position; Royce did not confine truth to the "merely logical." Royce was trying to characterize truth in a way sufficiently broad that all human interests, including a supreme purpose for individual life itself, might come within its scope. Consequently he chose the purpose, aim, and fulfillment schema as one capable of application to *any* idea conceived as a purpose. Cotton claims that Royce's analysis is a very "clear and convincing statement of the purpose of thought" (p. 87) when applied to strictly "logical purposes," by which I take him to mean the purpose of gaining theoretical knowledge, but that applied to other purposes, "organic, emotive, personal, whimsical," the theory "leads to bewildering results" (*ibid.*). I must dissent from this view. After all, a whimsey is known to be precisely such in virtue of the fact that, among other features, the object of its aim is not to be met with, i.e., its aim is not fulfilled, in the world of publicly accessible objects. Or, to take another illustration, since Royce believed in the necessity of order in moral relations, a purely self-regarding purpose will meet defeat in the world of external meanings (i.e., it will be a false purpose) to the extent to

which the true structure of human life precludes the possibility of its being lived on the basis of individual selfishness as a normative principle.

I do not want to minimize Royce's "intellectualism"—it is more extensive than he was aware—nor do I want to overlook the serious difficulties for the theory of the self raised by it; but neither do I care to lose sight of Royce's important and much-needed attempt to broaden the concept of truth so as to include within it the whole range of human endeavor. When Royce elevated being "rational" above all other aims, he did *not* mean the purely logical, as Cotton suggests; instead he was reviving the ancient conception of reason as order, measure, and form in all domains. Consequently his logic makes order basic, but, as he stressed so often, order is, or rather should be, exhibited in morality, art, and religion no less than in physics and mathematics. In short, order is present in all human purposes.

As regards the relation between immediacy and mediation in the grade of consciousness possessed by the absolute self, this is a complex problem which is ultimately related to an ambiguity in Royce's concept of completed truth. Is truth what Royce called in *The World and the Individual* the *least upper bound of a series of determinations* such that in possessing this limit the absolute self possesses *totum simul* what the entire series aims at, or is it rather, as suggested later in the theory of interpretation, *the complete system of interpretations* (including all errors and failures to grasp truth) which the ideal interpreter somehow knows both discursively and completely? Royce does not settle this question. Cotton claims that I have overstated the case (see n. 40, p. 321) in holding that the theory of interpretation completely eliminates the sort of immediacy characteristic of the *totum simul* insight of the Absolute in all of Royce's earlier writings. I agree with Cotton that the elimination of all immediacy in Royce's later thought is not only false to his position but destructive of all knowledge as well. Stress the discursive character of comparison, for example, as much as we will, there still remains an element of direct apprehension in knowing that two ideas are related in a given *respect* and that the resulting comparison depends completely upon the relevance of the respect chosen to each of the ideas compared.

But the point I was making in *Royce's Social Infinite*, and I think it is still valid, is that the early Royce was under the spell of Schopenhauer, and he thought of perfect knowledge as possessing above all an all-at-onceness. There is a change in Royce's thought as we move through and beyond the Gifford Lectures to a conception which stresses

less the absolute experience as immediacy and more the *system* of interpretations in which each sign is interpreted by another sign without end. As the theory of interpretation developed, Royce thought he had discovered the logical structure of a time-spanning knowledge or grasp of the relations between two ideas "from above." It is true, as the sentence quoted by Cotton from *The Problem of Christianity* (II, 271) explicitly states, that the idea of a "time-spanning insight" remains, but the insight is always *relational* (cf. the criticism of Bradley in the Supplementary Essay) and seeing the meaning of a set of ideas is not a bare immediacy but always involves the use of a "third" (cf. Royce's logical category "between"), which is precisely where the element of mediation comes in.

These comments are by no means adequate for conveying the care and completeness with which Cotton has presented the whole of Royce's thought. Nor, unfortunately, have I had time to discuss Cotton's account of Royce's ethical and religious ideas (pp. 238 ff.) and their culmination in the idea of the Beloved Community. Careful attention should be paid to these chapters, since they contain Royce's views, together with instructive criticism, on the ultimate destiny of the individual self to which the entire book directs attention.

JOHN E. SMITH

Yale University

THE ESSENCE OF PHILOSOPHY. By WILHELM DILTHEY. Translated into English by STEPHEN A. EMERY and WILLIAM T. EMERY. Chapel Hill, The University of North Carolina Press, 1954. Pp. xi, 78. Paper, $1.50; cloth, $2.50.

Dilthey's *Das Wesen der Philosophie* (first published in 1907) has been largely neglected, at least in this country, and in my opinion unfortunately so. It is here presented by the translators as "peculiarly fitted to introduce readers of English to Dilthey" and as "announcing themes elaborated in his other works: the relations of philosophy to religion and the fine arts, the types of *Weltanschauung*, the psychological and social basis of the history of philosophy, the positivistic critique of metaphysics" (p. ix). It is also fitted, one may add, to introduce such readers to a mature wisdom on these themes which is lacking in some current views of the nature and function of philosophy; and one may hope that the translation will serve this latter purpose also.

The assumption throughout the discussion is that the function of

philosophy springs "with inner necessity from the basic properties of mental life . . . is embedded in the structure of man . . . and every human activity tends to reach philosophical reflection" (p. 36). And this assumption is warranted *pari passu* in the discussion. Whether the warrant given is satisfactory remains open to doubt, largely because of Dilthey's antiquated psychology; but the assumption is fundamental.

The drive to philosophy is twofold: (a) the metaphysical urge to penetrate into the heart of the whole of the given world, "the tendency toward universality and logical grounding," and (b) the positivistic demand for universally valid knowledge—the urge to build *Weltanschauungen*, and the coercive quest for certainty. Positivism repudiates the former, mysticism repudiates the latter; both, however, are essential to philosophy.

This view is supported both by observation of the actual work of philosophers and by analysis of the activity of philosophizing. The historical survey discloses what is common to the various constructions of those called philosophers, the analysis discloses the nature and function of philosophizing in the cultural life, and both disclose the essentially dual nature of the philosophical enterprise. The contrariety of the two drives is thus disclosed also. And herein lies the perennial vitality of philosophy, the continuous construction of *Weltanschauungen* and their constant criticism and revision or even rejection in behalf of special problems: "the development of a *Weltanschauung* to universal validity; the reflection of knowing on itself; the relation of theories, formed in the particular purposive systems, to the system of all knowledge; a culturally pervasive spirit of criticism, of universal synthesis, and of grounding in principle"—these are all "particular activities which are rooted in the unitary essence of philosophy." Thus "we understand the continual change of its activities, the flexibility and mobility with which it now unfolds to the breadth of a system, now focuses its whole force effectively on a single problem, and is ever shifting its working energy to new tasks" (p. 75).

Thus, also, can we differentiate philosophy from the special sciences, poetry, and religion. "The tendency toward universality and logical grounding" is lacking in the sciences or, if pursued to its fulfillment, transforms them into philosophy. Likewise, the demand for universally valid knowledge is not met in poetry and religion or, if met, once more transforms them into philosophy. Thus, though different from philosophy, science and poetry and religion are alike imbued with the spirit of philosophy:

> It is present wherever investigators with a consciousness of method regress from their science to its ultimate grounds of justification or press forward to generalizations which connect and confirm several sciences. It is present wherever values of life and ideals are re-examined. . . . This spirit leaves no feeling of value and no striving in its immediacy, no precept and no knowledge in its isolation. For every valid thing it seeks the ground of its validity. In this sense the eighteenth century rightly called itself the philosophical century in virtue of its effective rule of reason over the dark, unconscious, instinctive forces within us and the regress from every historical product to its origin and its right [p. 73].

As Dilthey would presumably agree, there is nothing really novel in all of this. But its significance warrants its repetition, especially in such a mature and instructed variation as is here given to the central theme. And it is for this reason that I have summarized the position at some length—this, and also that I deem it sound in principle. The analysis is dated, of course, but it is by no means out of date; much of contemporary significance remains.

If the position is sound in principle, certain views of the nature and function of philosophy cannot stand or stand only with more or less drastic revision. Among them are: that philosophizing may be properly grounded in initial proposals and definitions; that dogmatic finality in philosophy is achievable; that skepticism is the only tenable philosophical attitude; that certainty, pure and undefiled, is the sole object of the philosopher's quest; that the emotional murkiness of the abstract present is his ultimate refuge; that philosophy on one side, and poetry and religion on the other, are logically disparate and have nothing to learn from each other; that the evaluative process is wholly without logical compulsion and therefore beyond the scope of philosophy strictly construed. The position, in short, contravenes the types of philosophy commonly called positivism, phenomenalism, skepticism, dogmatism, and logicism. Hence its perennial relevancy to philosophical issues, and especially to current controversies; that in these controversies it has not been given the serious consideration its importance deserves is closely linked with the thinnish sort of philosophizing which has latterly become à la mode.

The translation reads well, despite the rather awkward repetition of the terms "lived experience" and "human studies" (here adopted, following H. A. Hodges, as the English equivalents of "*Erlebnis*" and "*Geisteswissenschaften*" respectively). This could hardly have been avoided, however, in view of Dilthey's usage. Since I have not had access to the German text, I can make no comment about the fidelity of the translation.

G. Watts Cunningham

Cornell University

A MODERN PHILOSOPHY OF RELIGION. By SAMUEL M. THOMPSON. Chicago, Henry Regnery Company, 1955. Pp. xx, 601. $5.75.

This is a substantial work which should contribute to the current revival of interest on the part of many secular philosophers in the problems of religion. Before referring to its many merits, however, one must note as a demerit that the book does not justify the term "modern" in the title, at least not in the way which we should most naturally expect. For the distinctively modern queries in this field concern the nature of religious language, or rather of the religious uses of language, and the logical character of the theistic assertion. But this vista of problems is not brought within the scope of Dr. Thompson's discussion; and this, if not a vice, is at least the absence of a quite important virtue.

In spite of this, the book contains much that is of interest and profit for contemporary philosophers. For Thompson discusses most of the main topics in the philosophy of religion and has something illuminating to say about each of them. His general position is Thomist, although he draws subsidiarily upon a wide range of more recent thought.

He begins with a definition of religion considered as a human activity or disposition. Religion, he suggests, consists in regarding something as being supremely important and relating oneself to it and to everything else accordingly. The religious person "is one who has an attitude of acceptance and commitment to whatever he takes to be of ultimate value in existence" (p. 8). In this broad sense, as Thompson notes, an atheist may be religious; the devoted Marxist can be as religious as a Buddhist saint or Christian martyr. Although a phenomenological definition of this kind, containing no reference to the divine or the supernatural, is entirely feasible, it has the disadvantage that it postpones rather than brings forward the issues which one wants to consider. Better perhaps to define religion at the outset in terms which render its assertions both momentous and disputable. For it is not in the interests either of religion or the rejection of it to muffle its distinctive claims concerning the nature of the universe in which we live. Nor indeed does Thompson permit his initial definition to restrict the discussion for long: he proceeds quite soon from religion in general to theistic religion, dealing in the greater part of his book with the highly important issue between theism and naturalism.

From the definition of religion Thompson turns to the epistemological topic of the nature of religious faith. Here he takes up a voluntarist position. "The distinctive feature of faith," he says, "in contrast with

mere belief, is the element in it of will and action" (p. 74). This is a view which traces its ancestry to St. Thomas, and which has been developed as a doctrine of philosophical theology by F. R. Tennant. But in spite of its respectable lineage, it is open to serious criticism. The word "faith" occurs in religious language with two different uses: as *fides* and as *fiducia*, as the believer's cognition of God and as his practical attitude of trust toward God. Faith in the latter sense, being resolute self-commitment, invites a voluntarist analysis. But can faith in the former sense, as the (putative) awareness of the divine, be treated as an act of will without thereby abandoning its cognitive claim? Thompson does not sufficiently meet this difficulty in his discussion.

There follow three parts whose subjects I have space only to list, dealing with the term "truth" in relation to religion; the nature of man as the religious animal; and the three main conceptions of deity—pantheism, deism, and theism. Each of these sections contains a wealth of interesting and useful matter.

The next part, on the existence of God, is the core of the book. It sets forth fully, clearly, and persuasively the theistic argument *e contingentia mundi*, known also as the Cosmological Argument. This (St. Thomas' Third Way) is the argument from the contingency of the world to the existence of a necessary (or "self-existent") Being as its ground. It is distinguished from the Ontological Argument (as criticized by Kant) in having an empirical premise, namely the existence of the world; and from the Design Argument (as criticized by Hume) by proceeding not from *what* the world is, but from the bare fact *that* it is and that it is a realm of contingency.

The argument runs as follows: Whatever changes, existing at one time and not at another, is contingent, i.e., it is something which can either exist or not exist. Since everything in the world is thus contingent, there must have been a time when nothing existed. But in that case nothing could ever have come into existence; for there would have been nothing to produce it. There must then be something necessary, i.e., incapable of not existing, and this we call God. This is the traditional form of the argument; but it can equally well, and indeed perhaps better, be stated without reference to a time when there was no contingent existence. The basic principle of the argument, that in order for there to be anything contingent there must be something noncontingent, is compatible with almost any view of the nature of time, change, and causality.

Thompson discusses, and I think meets, the common objection that whilst each item in the world, considered separately, is contingent,

these items may together constitute a whole which is not contingent. He points out in rebuttal that "the world as a whole" has no existence other than as the aggregate of its parts. He also refers to the objection that the cosmological argument assumes the impossibility of an infinite series—in this case an infinite series of contingent events. Thompson seeks to answer this by insisting that a real, as distinguished from a mathematical, infinity is inconceivable. But this is debatable. A stronger reply would perhaps be that the kind of infinite series which the argument denies is not a temporal series at all, but rather what might be called a series of ontological dependence. No multiplication of contingent causes, it might be said, not even a multiplication to infinity, would avoid the need for a noncontingent ground of the contingent world. But all these considerations are subordinate to the more difficult and fundamental query as to whether, and in precisely what sense, the contingent-necessary dichotomy can meaningfully be applied not only to propositions but also to objects, and in particular to the unique object constituted by the world as a whole. This is the heart of the problem, and I cannot feel that Thompson has sufficiently investigated it in his book. However, those of us who are unable to accept the argument from contingency must nevertheless acknowledge its importance in the development of philosophical theology and may well be grateful to Thompson for his thorough and lucid presentation of it.

An important limitation of the argument from contingency, even supposing it to be valid, is that the Being whose existence it professes to prove is not the God of living religion. No one worships at the shrine of Necessary Existence. This is not the personal God of the Bible, "the God of Abraham, of Isaac, and of Jacob," "the God and Father of our Lord Jesus Christ." Thompson acknowledges this limitation, and he has a further Part, employing the method of analogy, in which he seeks to fill in the character of the God whose bare existence as Necessary Being he claims to have demonstrated. There is also a final section on God and the World, dealing interestingly with the subjects of creation, history, evil, and immortality.

In its form the book is written as a student's textbook, with questions and a reading list provided for each chapter. As a textbook, however, it suffers from being diffuse, and it is insufficiently informative as to the history of the subject and the classical discussions. But as a contribution to the philosophical scrutiny of religion, Thompson's book is a valuable publication.

JOHN H. HICK

Cornell University

CHRISTIAN ETHICS. Sources of the Living Tradition. Edited with Introductions by WALDO BEACH and H. RICHARD NIEBUHR. New York, The Ronald Press Co., 1955. Pp. 496. $5.00.

This is a book of selected readings from the "sources of the living tradition" of Christian ethics, designed to be used as a text in college and introductory seminary courses. Nevertheless, the introductory comments are ample, totaling (with the opening and the final chapters) 185 pages out of the 490 pages of text. The book provides, therefore, a satisfactory introduction to the materials of Christian ethics through both primary sources and a connected secondary interpretation, which, however, does not attempt original analysis of its own. Richard Niebuhr, Sterling Professor of Theology and Christian Ethics at Yale University, wrote the opening chapter on Biblical ethics and edited and commented on the selections from Luther, Calvin, and Jonathan Edwards. A former student of his and the initiator of this project, Waldo Beach, who is Professor of Christian Ethics at Duke University, did the rest, except that Professor Paul Holmer of the University of Minnesota guest edited the selections from Kierkegaard.

Following the elaboration of Biblical themes in the first chapter, the ethical teachings of the early church during its first century are represented by selections from the *Didache*, the Letter of St. Clement to the Corinthians, and the anonymous Letter to Diognetus; and the second century, by "The Instructor" and "The Rich Man's Salvation" by Clement of Alexandria. St. Augustine and St. Thomas Aquinas, of course, stand as summits whose approaches are, in the case of the former, selections from *Of the Morals of the Catholic Church* and from Books XII, XIV, and XIX of *The City of God*, and, in the case of the latter, selections from the *Summa Theologica*. Between these two there are chapters on the ethics of monasticism (*The Rule of St. Benedict* and *The Little Flowers of St. Francis*) and the ethics of mysticism (*On the Love of God*, by Bernard of Clairvaux, and selections from "About Disinterest" and the sermons of Meister Eckhart).

The reader will be grateful that Martin Luther is allowed to speak through his exposition of the ten commandments in *The Larger Catechism* rather than through the familiar treatise on Christian liberty. The selections from Calvin's *Institutes* concentrate on man's nature, the functions of the moral law, the Christian life, and civil government (including the epoch-shaking passage on "popular magistrates"). Baxter's *Christian Directory* and Barclay's *Apology* are spokesmen for the ethics of Puritanism and Quakerism in the seventeenth century, while the sermons of Joseph Butler represent Anglicanism and rationalism in

the eighteenth century. In that same century, Wesley's *Christian Perfection* and Edwards' *Nature of True Virtue* voice more orthodox and evangelical points of view. The selections from Kierkegaard are taken from quite a number of his writings. Since he was, however provocative today, essentially a "nineteenth-century spokesman of a twentieth-century view," the main currents of Christian thought in the nineteenth century are not represented here. The volume concludes with selections from Walter Rauschenbusch's *Theology for the Social Gospel* (his social Christianity contrasting with Kierkegaard's supposed individualism) and with a chapter on "Current Trends," in which are discussed the present-day revival of persistent historical traditions, such as neo-Thomism, neo-Natural law, and neo-orthodoxy, and the ethics of the ecumenical movement. Throughout all these writings appropriate section headings which will greatly assist the student have been introduced at frequent intervals by the editors.

With this survey of its contents the responsibility of a reviewer of such a volume as this has been discharged. He may further remark, however, that the publication of this book may prove an event of considerable importance if it serves not only as an anthology for use as a textbook but also as a primer by which the general and the educated reader is introduced to the strongest single and most persistent living moral tradition of the western world. The illusion has not yet been entirely dispelled from the minds of many of our colleagues in the universities that Christian ethics is all a matter of bad heteronomy or of what Kierkegaard called "the reward disease." Yet these objectionable emphases were never dominant in Biblical ethics or in the best reflection upon morality in this tradition that has gone on since. This was evident from the unique ethical outlook introduced by "The Words" in the oldest document of ethical significance in the Bible, the Book of the Covenant (contrasted with "The Ordinances" it also contains, which were drawn largely from the Code of Hammurabi). From the beginning, Biblical ethics has known that the scope, meaning, and intensity of that *for which* men are morally responsible depends in large measure, perhaps entirely, upon *to whom* they are responsible or responsive. Hence, love to God and to one's neighbor or companion in God under the rule of His will and according to the measure of His mercy constitutes the very definition of obligation, on which philosophy has never made any improvement. Speculation about values that are the elected ends of human moral endeavor is of only secondary importance, however much this may or may not illumine the goals men should also seek for those to whom they are morally bound and

engaged. This comment concerns only "the one in the many" varieties of Christian moral philosophy that have been developed over the centuries. Their variety, in turn, shows that Christian ethics is not a matter of barren reiteration, and of this the present volume gives ample evidence.

PAUL RAMSEY

Princeton University

HISTORY OF INDIAN PHILOSOPHY. Volume V. Southern Schoolsof Śaivism. By SURENDRANATH DASGUPTA. New York, Cambridge University Press, 1955. Pp. xiv, 204. $5.00.

The long-awaited Volume V of the late Professor Dasgupta's *History of Indian Philosophy* has at last made its appearance, although only in part. It is, however, said to be "more than a third of the proposed work," presenting a complete survey of the different schools of Southern Śaivism. The work is said to be mainly based on manuscripts collected from various sources in South India as well as on those rare manuscripts of *Śaivāgamas* which the author himself claims to have had in his possession as "completed wholes" or in "fragmentary portions" (p. 16). Several works on the Southern Śaivism, however, exist in the Dravidian languages like Tamil; and this has naturally made the task of the author more arduous than it was in his other volumes. The author, however, assures us that "he has taken good care to ascertain that the works in Dravidian, pertinent to the subject, are well represented in the Sanskrit manuscripts" (p. 16). It is also the author's belief that "there is hardly anything of philosophical value or systematic thought which is available in Dravidian, and not available in Sanskrit" (p. 16, footnote).

The book consists of five chapters: The first chapter is on the available *Śaivāgamas* which are the basic sources of the Southern Śaivism. Although the author is mainly concerned with the philosophical thought of Śaivism, he does not leave religious dogmas out of account, as they are inseparably connected with philosophy in Śaivism as in Śāktaism. He is content to take a few specimens of the Āgamas and discusses them even at the risk of "indulging in repetitions." The survey of the Āgamic literature, it is believed, "would give the reader the opportunity of judging the literary contents of the philosophical aspects of some of the important Āgamas and thereby getting a comprehensive view of the internal relation of Śaivism to other branches of Indian

philosophy" (p. 21). The second and the third chapters deal respectively with the history and literature of Vīra Śaivism and the type of Śaivism represented by Srikantha in his *Commentary* on the *Brahma Sūtra* and the *Sub-commentary* on it by Appaya Dixita. The fourth chapter is on the Śaiva philosophy in the *Purāṇas*. The author has chosen *Śiva Mahāpurāṇa* as a representative specimen and has devoted special attention to its last section, viz., the *Vāyavīya Samhitā*, which is one of the important early documents on southern Śaivism that are available. The last chapter is captioned "Śaiva Philosophy in Some of the Important Texts." The texts selected for study are: (a) the *Pāśupata Sūtras* (edited under Travancore Sanskrit Series), (b) *Tiru-vāchakam*, a Tamil hymnal work by a celebrated Śaiva saint, (c) *Tattva-Prakāshikā*, by King Bhoja, with its two commentaries, and lastly, (d) *Śrikarabhāṣya*, of Śripati Pandita, a reputed Vīra Śaiva commentary on the *Brahma Sūtra*.

It is needless to say that the work is a very valuable contribution to the study of Indian thought and culture. Southern Śaivism has been a neglected field of study in spite of the enormous amount of literature published and otherwise available on the subject. The author has endeavored to give a connected conspectus of the available Sanskrit works on the subject. There is a central philosophic theme running through the different phases of Śaivism, viz., the presence of a transcendent source of being and meaning in every object. The emphasis on its transcendence is the keynote of Śaivism. The only method adequate to realizing the transcendent consists in rising above the empirical. The concepts of "*mahā samhāra*," "*Hara*," and so on are suggestive of this idea. There is no school of philosophical Śaivism, the Pāśupata included, that does not subscribe to this truth. On reading the author's survey, however, one is left with the impression that he has missed this wood for the trees. The author merely piles up statements, well-documented, no doubt, but without bearing upon any common theme. Consequently, failing to see any community of ideas, he has also failed to bring out the uniqueness of the standpoints of the various schools and subschools of Śaivism. What is still less justifiable, the author has mistaken at places the philosophy of Śaivism for an empirically grounded theism like that of the Nyāya, simply because some of the *Śaivāgamas* like *Mrigendra*, *Matanga Pārameśwara*, and *Śivajnānabodha* have adduced proofs for the existence of God. Meykanda Deva, it is said (p. 11), "tries to establish his system on the basis of inference." This is clearly a wrong statement, because Meykanda Deva himself in defining Śiva as Sat points out that by Sat is meant that which is not

known by any of the empirical means of knowledge (*Śivajnānabodham*, Sutra 6). The Sanskrit commentary available in the Graṇtha edition (the existence of which has, like many other important works, escaped the notice of the author) introduces a full-length debate in that context on the issue of the inferribility of God and adequately distinguishes the standpoint of Saiva Siddhānta from that of Nyāya. Similarly, the generality of Śaiva schools advocating some type of *advaita* maintains like the Vedanta that Siva is both the efficient and instrumental cause of the world. Śaiva Siddhānta, too, subscribes to this formula, though differing in interpretation from other schools. The author summarily treats them, and Śaiva Siddhānta in particular, as only advocating kevala-nimitta-vāda. He does not seem to be aware of the truth admitted by the Saivagamas themselves and demonstrated at great length by texts like *Śivajnānabodham* that Śaiva Siddhānta is as much an exegetical interpretation of Vedānta as it is of Āgamas, and that Śaiva Siddhānta advocates unqualified *advaita*.

The author's survey of the various Śaivagamas in the first chapter is the least satisfactory part of the work. The accounts are very sketchy and repetitive and sometimes even misleading (as for example when he says that "the *Mrigendra Āgama* opens the discussion of how the Vedic forms of worship became *superseded* by the Śaiva cult" [p. 21]). This is not at all surprising because, firstly, the author has not utilized the extensive commentaries on these Āgamas available in manuscript form or in the Graṇtha editions. In particular, the elaborate Bhāṣya on *Pauṣkara Āgama* by Umāpati and the one on *Sivajnāna Bodha* by Śivāgrayogin alone would have enabled the author to reconstruct the metaphysics of the Śaivagamas with greater clarity and insight. There are standard editions (Graṇtha) of the two commentaries available with introduction and notes and it is surprising that they have escaped the vigilance of the author. Secondly, what is still more deplorable, the author seems to be oblivious of the existence of several important manuals on Śaiva siddhānta by writers like Sadyojyoti, Srikaṇtha, and others which purport to give a bird's-eye view of the *vidyā-pādas* of the Āgamas like *Raurava* and the *Svāyambhuva*. Sadyojyoti's *Bhoga-kārikā* and *Mokṣa karikā* taken together represent an exhaustive and definitive restatement of the philosophy of Śaivagamas. Similarly, Śrikantha's *Ratna Traya* is a sustained discussion of the interrelationship of the three central concepts of Bindu, Śakti, and Śiva. Nārāyaṇakaṇtha's *Nādakārikā* is an excellent introduction to the Śaiva Siddhānta philosophy of grammar. It is very surprising that in an "extensive study of Southern Śaivism based on original Sanskrit

texts" there is no mention of the theory of Nāda and its implication for the metaphysics of Śaivism. All these aforementioned works, and many others like Sarvātma Śambhu's *Siddhānta Prakāshikā*, and Sri Surya Siva's *Śaiva Paribhāṣa* are available with commentaries in Devanagari editions! Even admitting the truth of the statement that there is hardly anything of philosophical value available in Tamil which is not available in Sanskrit on Śaivism, one feels the author has failed to do full justice even to the limited task which he set himself.

Besides these serious omissions there are other inaccuracies and errors of commission, hardly becoming a meticulous historian of Indian philosophy. "Umāpati," it is said (p. 19) "is the author of *Pauṣkara Āgama*." Umāpati has only a commentary on that Āgama. Similarly, at other places it is said that "Umāpati has a commentary on *Śiva Jnāna Bodha* (p. 151) and that "Umāpati interprets *Tiruvāchakam*" (p. 156), etc. Umāpati is one of the three who have written original Tamil works on Śaiva-Siddhānta and his works are independent treatises, though certainly inspired by *Śivajnāna Bodham*. Regarding his alleged interpretation of *Tiruvāchakam*, his *Tiruvarutpayan*, which the author seems to have in view in this connection (pp. 156, 157), is no more a commentary on *Tiruvāchakam* than *Advaita Siddhi* could be a commentary on *Brihadāraṇyaka Upanishad*! Dr. Pope in his translation of *Tiruvāchakam* gives a full-length exposition of Umāpati's aforementioned treatise on Śaiva Siddhānta as a useful introduction to the Siddhānta philosophy which is implied in the hymns of *Tiruvāchakam*. The author is evidently misled into thinking that Umāpati is interpreting *Tiruvāchakam*. "Aghora Śivacharya's *Paśupati Pāśa Vichāra Prakarana*," it is said (p. 26), "discusses the main topics of *Śivajnāna-bodham*." This is chronologically not sustainable. *Sivajnāna asiddhi*, a Tamil metrical work closely modeled on *Sivajnāna bodham*, is mistaken at places for *Jnāna Siddhi*, a minor Āgama, the manuscript of which seems to have been available to the author.

To these errors of information may be added errors of interpretation which greatly disfigure the work of so eminent a scholar. On page 23 it is said that "thirty-six *kalās* are produced from the disturbance of Māyā." It should be thirty-six *tattvas*. Nor is "*kalās*" a misprint for *tattvas*, for the author then proceeds to define the meaning of *kalās*, where again unfortunately he seems to confuse between an evolute *(pariṇama)* of *Aśuddha māyā* and emanation *(vritti)* of *Bindu*. *Kalā* is used in two different senses in Śaivagamas and in neither sense, of course, could it be used as convertible with *tattva*. Again, while making a distinction between Śaiva Siddhānta and "other sectarian Śaivas" (p. 151), the

author says that according to the former the innate corruption of the soul always remains in the soul as a "permanent dark spot." "So the *personal identity and the imperfections cling together* in all finite beings and *they are never destroyed even in liberation*" (italics our own). This can be neither legitimate exposition nor legitimate criticism. The Śaiva Siddhānta envisages the continuance of *mala* in a very different sense and also makes a distinction between an existent and a reality.

The book is not without its merits. The author has explored regions not treated by indologists and has given us valuable information. His chapter on *Śiva Mahāpurāṇa* is easily the best part of the work, even though one may not agree with his reading of two dissimilar types of Śaivism in the earlier and the later portions of *Vāyaviya Samhitā*. The author could have also devoted a subsection on *Sūta Samhitā*, to which he makes useful references at many places. His treatment of Śrikantha's Śaivism and of Vīra Śaivism of *Prabhulingalilā*, *Anubhavā-sūtra*, and *Śrikara Bhāṣya* is fair and makes refreshing reading. The author complains unjustly that Śrikaṇṭha has not received any attention from writers on Southern Śaivism. The late S. Surya narayana Sastri's *Śivādvaita of Śrikaṇṭha*, published by the Madras University (1932), is still one of the major contributions to the study of Śaivism made in the recent past.

B. L. Atreya
K. Sivaraman

Banaras Hindu University

BOOKS RECEIVED

Santayana and the Sense of Beauty. By Willard E. Arnett. Bloomington, Indiana University Press, 1955. Pp. xv, 252. $4.50.

The Problem of Free Choice. By St. Augustine. Translated and Annotated by Dom Mark Pontifex. Westminster, Md., The Newman Press, 1955. Pp. vi, 92. $3.25.

La Valeur dans l'histoire. By Felice Battaglia. Translated by M.-L. Roure. Paris, Aubier, 1955. Pp. 206. Fr. 525.

The Philosophy of Epictetus. By John Bonforte. New York, Philosophical Library, 1955. Pp. xiv, 146. $3.00.

Human Personality and the Possibility of Its Survival. The Agnes E. and Constantine E. A. Foerster Lecture on the Immortality of the Soul. By C. D. Broad. Berkeley, University of California Press, 1955. Pp. 27. $.50.

Subject and Object in Modern Theology. The Croall Lectures Given in the University of Edinburgh, 1953. By James Brown. New York, The Macmillan Company, 1955. Pp. 214. $3.75.

Religio Medici. By Sir Thomas Browne. New York, Cambridge University Press, 1955. Pp. xxi, 102. $1.75.—A pocket edition of the text established by Jean-Jacques Denonain with a biographical and critical introduction.

Studies in Comparative Æsthetics. By Pravasjivan Chaudhury. Santiniketan, India, Visva-Bharati, 1953. Pp. iii, 127. 5 rupees.

A History of Indian Philosophy. Vol. V, Southern Schools of Śaivism. By the late Surendranath Dasgupta. New York, Cambridge University Press, 1955. Pp. xiv, 204. $5.00.

New Essays in Philosophical Theology. Edited by Antony Flew and Alasdair MacIntyre. New York, The Macmillan Company, 1955. Pp. xii, 274. $4.75.

Grundlagen der Ontologie Nicolai Hartmanns. Eine kritische Untersuchung. By Caspar Toni Frey. Tübingen, Max Niemeyer Verlag, 1955. Pp. 68. DM 4.–.

Debates with Historians. By Pieter Geyl. New York, Philosophical Library, 1956. Pp. viii, 241. $7.50.

Vorträge und Aufsätze. By Martin Heidegger. Pfullingen, Verlag Günther Neske, 1954. Pp. 284. DM 16.80.

International Encyclopedia of Unified Science. Vol. I, Parts 1 and 2. Edited by Otto Neurath, Rudolf Carnap, and Charles W. Morris. Chicago, University of Chicago Press, 1955. Pp. ix, 760. $6.00 per copy; $11.00 per set.

L'Un et le multiple. De la critique à l'ontologie. By Jacques Jalabert. Paris, Presses Universitaires de France, 1955. Pp. 166.

The Classics and Renaissance Thought. Martin Classical Lectures, Vol. XV. By Paul Oskar Kristeller. Cambridge, Harvard University Press, 1955. Pp. 106. $2.50.

Our Philosophical Traditions. A Brief History of Philosophy in Western Civilization. By Sterling P. Lamprecht. New York, Appleton-Century-Crofts, Inc., 1955. Pp. xi, 523. $5.50.

Language in Relation to a Unified Theory of the Structure of Human Behavior. Part II. By Kenneth L. Pike. Glendale, Calif., Summer Institute of Linguistics, 1955. Pp. v, 85. $2.00.

L'esigenza spiritualista nella politica della francia postrivoluzionaria. By Goffredo Quadri. Firenze, La Nuova Italia. Pp. 308. L. 1200.

La conoscenza storica. Analisi della logica Crociana. By Renzo Raggiunti. Firenze, La Nuova Italia, 1955. Pp. 309.

A Lasting Peace and the State of War. By Jean Jacques Rousseau. Translated by C. E. Vaughan. New Haven, Whitlock's, Inc. Pp. xxvii, 36-128. $2.00.

Justus Lipsius. The Philosophy of Renaissance Stoicism. By Jason Lewis Saunders. New York, Liberal Arts Press, 1955. Pp. xviii, 228. $4.50.

Tulane Studies in Philosophy. Vol. IV. Studies in American Philosophy. New Orleans, Tulane University, 1955. Pp. 96. $2.00.

Filosofia e scienza. By Giovanni Tuni. Firenze, La Nuova Italia, 1955. Pp. 99.

Ein System der negationslosen Logik mit ausschliesslich realisierbaren Prädikaten. Acta Philosophica Fennica. Fasc. IX, 1955. By Veli Valpola. Helsinki, Societas Philosophica, 1955. Pp. 247.

L'estetica di Hegel. Saggio di interpretazione filosifica. By Giovanni Vecchi. Milano, Vita E Pensiero. Pp. vii, 243. L. 2000.

La logica di Aristotele. By Carlo Augusto Viano. Torino, Casa Editrice Taylor, 1955. Pp. 314. L. 2000.

Theodor Litt. By Paul Vogel. Berlin, Walter De Gruyter & Co., 1955. Pp. 54. DM 4.80.

Plato's Philosophy of Mathematics. By Anders Webderg. Stockholm, Almquist & Wiksell, 1955. Pp. 154. Sw. Cr. 19:—.

* * * * *

The Supreme Doctrine. Psychological Studies in Zen Thought. By Hubert Benoit. Foreword by Aldous Huxley. New York, Pantheon Books, 1955. Pp. xv, 248. $4.50.

Love and Love's Philosophy. A Contribution to the Biology of the Personality. By Henri Birnbaum. New York, Pageant Press, Inc., 1955. Pp. 236. $3.50.

Cosmic Design. Man's Inner Urge for Progress. By Meyer N. Bruskovsky. New York, Vantage Press, 1955. Pp. 129. $2.00.

What I Have Learned by Living. By Henry J. Burt. Boston, Bruce Humphries, Inc., 1955. Pp. 147. $3.00.

American Literature and the Dream. By Frederick I. Carpenter. New York, Philosophical Library, Inc., 1955. Pp. vii, 220. $4.75.

Christianity and Freedom. A Symposium. New York, Philosophical Library, 1956. Pp. xi, 163. $2.75.

Energy and Structure in Psychoanalysis. By Kenneth Mark Colby. New York, Ronald Press Company, 1955. Pp. ix, 154. $4.50.

The French Faust: Henri de Saint-Simon. By Mathurin Dondo. New York, Philosophical Library, 1955. Pp. 253. $3.75.

Postulates and Implications. By Ray H. Dotterer. New York, Philosophical Library, 1955. Pp. 509. $7.50.

Martin Buber: The Life of Dialogue. By Maurice S. Friedman. Chicago, University of Chicago Press, 1955. Pp. x, 310. $6.00.

Reinhold Niebuhr: His Religious, Social, and Political Thought. Edited by Charles W. Kegley and Robert W. Bretall. New York, The Macmillan Company, 1956. Pp. xiv, 486. $6.50.

Moral Principles in the Bible. A Study of the Contribution of the Bible to a Moral Philosophy. By Ben Kimpel. New York, Philosophical Library, 1956. Pp. 172. $4.50.

The Philosophy of Time. By Dora Marsden. Oxford, The Holywell Press, Ltd., 1955. Pp. 34. 5s.

Maritain on the Nature of Man in a Christian Democracy. By Norah Willis Michener. Hull, Canada, Editions "L'Éclair," 1955. Pp. xv, 149. $4.25.

Geschichte der Autobiographie. Vol. II, Part I: Das Mittelalter. By Georg Misch. Frankfurt Am Main, Verlag G. Schulte-Bulmke, 1955. Pp. 666. Paper, DM 37.–; cloth, DM 40.—.

Speculative Instruments. By I. A. Richards. Chicago, University of Chicago Press, 1955. Pp. xii, 216. $4.50.

La teologia esistenzialistica di Karl Barth. Analisi, interpretazione e discussione del sistema. By Emmanuele Riverso. Napoli, Istituto Editoriale Del Mezzogiorno, 1955. Pp. 425. L. 3000.

Griechische Mythologie. Ein Handbuch. By Herbert Jennings Rose. München, C. H. Beck'sche Verlagsbuchhandlung, 1955. Pp. xi, 364. Paper, DM 17.—; cloth, DM 21.—.

On the Nature of Man. An Essay in Primitive Philosophy. By Dagobert D. Runes. New York, Philosophical Library, 1956. Pp. 105. $3.00.

Science and Modern Life. By Sir E. John Russell. New York, Philosophical Library, 1955. Pp. 101. $2.75.

Freud and the Crisis of Our Culture. By Lionel Trilling. Boston, Beacon Press, 1955. Pp. 59. $1.00.

Two Essays. Art, Nature, Education and *The Husbandry of the Spirit.* By Gretchen Warren. Oxford, Basil Blackwell, 1955. Pp. xiv, 58. 7s. 6d.

Thinking about Thinking. By Merl Ruskin Wolfard. New York, Philosophical Library, 1955. Pp. x, 273. $5.00.

Yoga Dictionary. By Ernest Wood. New York, Philosophical Library, 1956. Pp. xi, 178. $3.75.

Man in This World. By Hans Zehrer. New York, New York University Press, 1955. Pp. 317. $4.95.

* * * * *

Selected Writings on Religion and Society. By Edward Bellamy. Edited with an Introduction by Joseph Schiffman. New York, Liberal Arts Press, 1955. Pp. xlix, 139. $.75.

The Common Sense of the Exact Sciences. By William Kingdom Clifford. Edited and with a Preface by Karl Pearson. Newly Edited and with an Introduction by James R. Newman. Preface by Bertrand Russell. New York, Dover Publications, Inc., 1955. Pp. lxvi, 249. $1.60 (paper).

Matter and Light. The New Physics. By Louis de Broglie. Translated by W. H. Johnston. New York, Dover Publications. Pp. 300. $1.60 (paper).

Essays in the History of Ideas. By Arthur O. Lovejoy. New York, George Braziller, Inc., 1955. Pp. xv, 359. $3.75.

The Communist Manifesto. With selections from *The Eighteenth Brumaire of Louis Bonaparte and Capital*. By Karl Marx and Friedrich Engels. Edited by Samuel H. Beer. New York, Appleton-Century-Crofts, Inc., 1955. Pp. xxx, 96. $.45.

The Social Psychology of George Herbert Mead. Edited and with an Introduction by Anselm Strauss. Chicago, University of Chicago Press, 1956. Pp. xvi, 296. $1.50.

NOTES

The following officers of the Charles S. Peirce Society were elected at the recent annual meeting:

President	Professor Philip P. Wiener City College of New York
Member of Executive Committee	Professor Thomas A. Goudge University of Toronto
Secretary-Treasurer	Professor Edward H. Madden University of Connecticut.

THE SOCIETY FOR PSYCHICAL RESEARCH PRIZE ESSAY

The excellent response to last year's essay competition (which was won by Mr. William E. Cox, Jr.) has encouraged the donor to repeat his offer, and a prize of fifty pounds (or $140) will be awarded for an essay of less than 3,000 words on one of the following topics :

(a) The best program of research for the first one hundred hours of work with the next successful card-guessing subject discovered (if there is one): the design should be supported by argument.

(b) The best discussion of the logical and/or scientific implications of precognition, assuming this to be established statistically from the work of card-guessing subjects.

Entries should be submitted under a pseudonym together with a sealed envelope containing the entrant's actual name and address. They should be carefully written or typewritten, double spaced, and should reach the Secretary, Society for Psychical Research, 31 Tavistock Square, London W.C. 1, England, before October 1, 1956. Entrants may submit more than one entry. The judges will be Dr. D. J. West, Professor H. H. Price, and Mr. Denys Parsons, with assistance from Dr. S. G. Soal.

Printed in Belgium

Two new titles in the series, *Cornell Studies in Civil Liberty*

CIVIL LIBERTY IN THE UNITED STATES

A Guide to Current Problems and Experience

By Robert E. Cushman
Cornell University

A temperate, concise presentation by a leading political scientist of where we stand today on all aspects of the problems of civil liberties and civil rights. Recent court decisions, legislation, and experience is included.

261 pages, $2.85

FREEDOM'S FETTERS:

The Alien and Sedition Laws and American Civil Liberties

By James Morton Smith
Inst. of Early Amer. Hist. and Culture

The early development of our civil liberties is illuminated in this thorough analysis of the turmoil that accompanied the enactment of the Alien and Sedition Laws during John Adam's administration. Published for the Institute of Early American History & Culture.

484 pages, $5.00

CORNELL UNIVERSITY PRESS, *Ithaca, N.Y.*

THE INTERNATIONAL INSTITUTE OF PHILOSOPHY

announces, beginning January, 1954, a new form of the international bibliography

BIBLIOGRAPHY *of* PHILOSOPHY

Quarterly Bulletin

published for the International Federation of Philosophical Societies under the auspices of the International Council of Philosophy and Humanistic Studies with the aid of UNESCO and of the French National Center for Scientific Research

Provides abstracts, translated into either English or French where necessary, of books in all branches of philosophy that are published in countries in correspondence with the I.I.P.

To secure a complete file, subscriptions should begin with January, 1954. Continuation volumes of the annual bibliography for 1951 and 1952-53 will appear shortly.

Annual subscription: $3.00. Special rate to members of associations affiliated with the I.I.P. $2.75. Single copies, $1.00. Subscriptions may be sent to Professor Harold A. Larrabee, Director, Editorial Center, Bailey Hall 207, Union College, Schenectady, New York, U.S.A., or to

LIBRAIRIE PHILOSOPHIQUE J. VRIN
6, Place de la Sorbonne, Paris V^e^, France

Winston W. Little
W. Harold Wilson
W. Edgar Moore

APPLIED LOGIC

"here is logic for use"

"a readable, straigthforward account of the basic principles with a wealth of illustrative material. Here is logic for use. Students should find it a valuable introduction to the science and art of effective thinking."

Lewis E. Hahn
Washington University

Lucius Garvin

A MODERN INTRODUCTION TO ETHICS

"avoids the wishy-washy and the adulterated"

"This book seems to me by far the best ethics text available today, and indeed, the best that has appeared in a long, long time. ... (Garvin) avoids the wishy-washy impression given by so many who try to leave themselves entirely out of their presentation. But he also avoids the common evils of the opposite course, adulterating the views of others by an admixture of one's own, and centering one's work on a presentation and defense of one's own theory. Garvin, though he lets his views be known, is always careful to separate them from the theories he is discussing... Rare indeed is such a temperate and rational discussion of crucial problems..."

Charles A. Baylis
in ***The Journal of Philosophy***

HOUGHTON MIFFLIN COMPANY

• Contemporary European Philosophy

By I. M. Bochenski

Translated by Donald Nicholl and Karl Aschenbrenner. An introductory survey of the major contemporary European thinkers, presenting their basic concepts in their own terms. This first edition in English is slightly revised from the original German.

344 pages August $5.00

• Whitehead's Philosophical Development

By Nathaniel Lawrence

Analytic review of Alfred North Whitehead's thought, designed as an introduction to the study of his central work, *Process and Reality*. Chronological but not merely historical, Lawrence's interpretation follows Whitehead through "the steady change of mind" which characterized the development of his philosophical synthesis.

394 pages August $5.00

• The Direction of Time

By Hans Reichenbach

The last work of a well-known philosopher of science. Contents include: Time Order of Mechanics, Time Direction of Thermodynamics and Microstatistics, Time Direction of Macrostatistics, and Time of Quantum Physics, as well as an introduction discussing the emotive significance of time.

288 pages $5.50

At your bookstore or

UNIVERSITY OF CALIFORNIA PRESS

Address: **Berkeley 4, California**

1906 1956

The 50th Anniversary of EVERYMAN'S LIBRARY

"The effect that this great collection of books has had upon the intellectual life of England and America is incalculable—indeed, it is hard to imagine how our intellectual life could be carried on without Everyman."—LIONEL TRILLING

LAST SPRING EVERYMAN'S LIBRARY, the world's largest collection of great books, reached an unprecedented goal—the publication of the 1000th volume in the series. From the great classics of antiquity down to Van Wyck Brooks' modern masterpiece, MAKERS AND FINDERS, Everyman's Library has for a half-century provided the English-reading world with the great works that are the foundation of a liberal education.

It was in 1906 that E. P. Dutton and Co., with J. M. Dent and Sons of London, launched this dynamic venture in publishing. Their aim—to make the world's great books available to every reader in well-edited, handy-sized, low-priced volumes. Boswell's LIFE OF DR. JOHNSON inaugurated the new series; and as soon as the first of these flat-backed, gold-ornamented volumes appeared on bookstands, the success of the project was assured.

"It is hard to imagine what college teaching in the humanities would have been during this half-century without this oldest and still by far the most comprehensive collection of the classics."—J. H. RANDALL, JR., Professor of Philosophy, Columbia University.

"Everyman's Library ... may well be termed one of the pillars of Anglo-American culture and of humane studies and ideals."—ALLAN NEVINS

Volume #1000

Aristotle's Metaphysics

A completely new translation, edited and prefaced by *John Warrington*. Introduced by *Sir David Ross*. #1000 $1.65

Other new titles this spring

The Betrothed

By Alessandro Manzoni. The magnificent Italian historical novel translated by *Archibald Colquhoun*. #999 $1.65

Everyman and Medieval Miracle Plays

New revised and enlarged edition prepared by *A. C. Cawley* #381 $1.65

Euripides: Plays

Newly translated by *A. S. Way*. #63, #271 each $1.65

Aeschylus: Plays

Newly translated by *G. M. Cookson*. #62 $1.65

Milton's Poems

New edition based on Milton's editions and manuscripts. Edited and introduced by *Prof. B. A. Wright*, University of Southampton. #384 $1.65

E. P. DUTTON & CO. • 300 Fourth Avenue • New York 10

Prentice-Hall

PHILOSOPHIC INQUIRY:
An Introduction to Philosophy

by LEWIS W. BECK, *Professor of Philosophy, University of Rochester*

This text invites the student to "think things through" as problems grow out of everyday life, conduct, science, and religion. It brings successively into focus:

a. inquiry and such substitutes as propaganda, blind faith, and appeals to authority

b. the distinctive traits of science and its philosophic consequences

c. the problems of ethics and conduct in the light of scientific method

d. the legitimacy of speculative philosophy and the nature of the major metaphysical systems in the Western world—theism, idealism, materialism, pragmatism, naturalism.

470 pages • 5-5/8" × 8-3/8"

CRITICAL THINKING
Second Edition
An Introduction to Logic, Semantics, and Scientific Method

by MAX BLACK, *Cornell University*

- Reduces technical theory to a minimum, expands study of topical examples from newspapers, books, advertisements.
- Without striving for novelty for its own sake the author has made a number of innovations: for example, the simplified treatment of syllogism and the transition from "simple enumeration" to more complex inductive methods.
- A serious effort, unusual in such texts, has been made to provide materials adapted to students of varying ability.

439 pages • 5-5/8" × 8-3/8"

For approval copies write
Prentice-Hall, Inc.